CARNEGIE INSTITUTION

OF

WASHINGTON

YEAR BOOK

No. 1

1902

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OFFICERS

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Daniel C. Gilman

Executive Committee

Daniel C. Gilman, *Chairman*
Charles D. Walcott, *Secretary*
John S. Billings
Abram S. Hewitt
S. Weir Mitchell
Elihu Root
Carroll D. Wright

Finance Committee

Lyman J. Gage
Henry L. Higginson
D. O. Mills

Office of the Institution
1439 K Street, Washington D. C.

Marcus Baker
Assistant Secretary, in Charge

ARTICLES OF INCORPORATION

OF THE

CARNEGIE INSTITUTION OF WASHINGTON.

We, the undersigned, persons of full age, and citizens of the United States, and a majority of whom are citizens of the District of Columbia, being desirous to establish and maintain, in the City of Washington, in the spirit of Washington, an institution for promoting original research in science, literature, and art, do hereby associate ourselves as a body corporate for said purpose, under An Act to establish a code of Law for the District of Columbia approved March third, nineteen hundred and one, sections 599 to 604 inclusive; and we do hereby certify in pursuance of said act as follows:

First. The name or title by which such institution shall be known in law is CARNEGIE INSTITUTION.

Second. The term for which said Institution is organized is perpetual.

Third. The particular business and objects of the Institution are the promotion of study and research, with power:

(*a*) To acquire, hold, and convey real estate and other property necessary for the purposes of the Institution as herein stated, and to establish general and special funds;

(*b*) To conduct, endow, and assist investigation in any department of science, literature, or art, and to this end to cooperate with governments, universities, colleges, technical schools, learned societies, and individuals;

(*c*) To appoint committees of experts to direct special lines of research;

(*d*) To publish and distribute documents;

(*e*) To conduct lectures;

(*f*) To hold meetings;

(*g*) To acquire and maintain a library;

(*h*) And, in general, to do and perform all things necessary to promote the objects of said Institution.

Fourth. That the affairs, funds, and property of the corporation shall be in general charge of a Board of Trustees, the number of whose members for the first year shall be twenty-seven (27), and shall not thereafter exceed thirty except by a three-fourths vote of said Board.

IN TESTIMONY WHEREOF we have hereto set our names and affixed our seals, at the City of Washington, in the District of Columbia, on the fourth day of January, 1902.

JOHN HAY [SEAL]
EDWARD D. WHITE [SEAL]
JOHN S. BILLINGS [SEAL]
DANIEL C. GILMAN [SEAL]
CHARLES D. WALCOTT [SEAL]
CARROLL D. WRIGHT [SEAL]

DISTRICT OF COLUMBIA: *ss:*

Be it remembered that on this 4th day of January, A. D. 1902, before the subscriber personally appeared the above named John Hay, Edward D. White, John S. Billings, Daniel C. Gilman, Charles D. Walcott, and Carroll D. Wright, to me personally known and known to me to be the persons whose names are subscribed to the foregoing instrument of writing, and severally and personally acknowledged the same to be their act and deed for the uses and purposes therein set forth.

Given under my hand and official seal the day and year above written.

WILLIAM MCNEIR,
Notary Public.

[SEAL]

BY LAWS

OF THE

CARNEGIE INSTITUTION OF WASHINGTON

[As amended and adopted November 25, 1902]

1. The officers of the Board of Trustees shall be a Chairman, a Vice Chairman, and a Secretary, all of whom shall be chosen biennially, by ballot.

2. The annual meeting of the Board shall be held in Washington, on the second Tuesday of December, beginning with the year 1903. Other meetings of the Board may be called by the Executive Committee on twenty days' notice to each member of the board, and they shall be called in the same manner by the Chairman of the Board or by the Secretary, on the written request of seven members of the Board.

3. The Trustees, by ballot, shall appoint a President of Carnegie Institution, whose term of office shall be five years. He may or may not be a member of the Board of Trustees.

4. The Trustees, by ballot, shall designate seven trustees as an Executive Committee. Their terms of office shall be three years, and the members shall be reëligible. The Committee shall determine by lot the term of office. Any person elected to fill a vacancy shall be chosen for the unexpired part of his predecessor's term.

5. The fiscal year of the Institution shall be from November 1 to October 31, inclusive.

6. There shall be a Finance Committee consisting of three members of the Board, to be elected by the Board, to hold office until their successors are elected. The duty of such Finance Committee shall be to consider and recommend to the Board of Trustees such measures as it may believe will promote the financial interests of the Institution.

7. The Executive Committee shall, when the Board is not in session and has not given specific directions, have charge of all arrangements for administration, research, and instruction; designate advisory committees for specific duties, determine all payments and salaries; keep a written record of all their transactions and expenditures, and submit to the Board of Trustees at the annual meeting a report,

The said gift is to be held in trust for the purposes hereinafter named or referred to, that is to say, for the purpose of applying the interest or annual income to be obtained from the said bonds or from any other securities which may be substituted for the same: for paying all the expenses which may be incurred in the administration of the Trust by the Trustees, including in said expenses the personal expenses which the Trustees may incur in attending meetings or otherwise in carrying out the business of the Trust: AND SECOND, for paying the sums required by the said Trustees to enable them to carry out the purposes hereafter expressed. I hereby confer on the Trustees all the powers and immunities conferred upon Trustees under the law, and without prejudice to this generality the following powers and immunities, viz.: Power to receive and realize the said Bonds, and the principal sums therein contained and the interest thereof, to grant discharges or receipts therefor, to sell the said Bonds, either by public sale or private bargain, at such prices and on such terms as they may deem reasonable, to assign or transfer the same, to sue for payment of the principal sums or interest, to invest the sums which from time to time may be received from the said Bonds on such securities as Trustees are authorized by the law of the State of New York, Pennsylvania, or Massachusetts, to invest Trust Funds,—and also on such other securities as they in the exercise of their own discretion may select, and to alter or vary the investments from time to time as they may think proper;

And I hereby expressly provide and declare that the Trustees shall to no extent and in no way be responsible for the safety of the said Bonds, or for the sums therein contained, or for the securities upon which the proceeds of the said Bonds may be invested, or for any depreciation in the value of the said Bonds or securities, or for the honesty or solvency of those to whom the same may be entrusted, relying, as I do, solely on the belief that the Trustees herein appointed and their successors, shall act honorably;

And I further hereby empower the Trustees to administer any other funds or property which may be donated or bequeathed to them for the purposes of the Trust; and I also empower them to appoint such officers as they may consider necessary for carrying on the business of the Trust, at such salaries or for such remuneration as they may consider proper, and to make such arrangements, and lay down from time to time such rules as to the signature of deeds, transfers, agreements, cheques, receipts, and other writings, as may secure the safe and convenient transaction of the financial business of the Trust. The Committee shall have the fullest power and discretion in dealing with the income of the Trust, and expending it in such manner as they think best fitted to promote the objects set forth in the following clauses:—

The purposes of the Trust are as follows, and the Revenues therefrom are to be devoted thereto:—

It is proposed to found in the city of Washington, an institution which with the co-operation of institutions now or hereafter established, there or elsewhere, shall in the broadest and most liberal manner encourage investigation, research, and discovery—show the application of knowledge to the improvement of mankind, provide such buildings, laboratories, books, and apparatus, as may be needed; and afford instruction of an advanced character to students properly qualified to profit thereby.

Among its aims are these:

1. To promote original research, paying great attention thereto as one of the most important of all departments.

2. To discover the exceptional man in every department of study whenever and wherever found, inside or outside of schools, and enable him to make the work for which he seems specially designed his life work.

3. To increase facilities for higher education.

4. To increase the efficiency of the Universities and other institutions of learning throughout the country, by utilizing and adding to their existing facilities and aiding teachers in the various institutions for experimental and other work, in these institutions as far as advisable.

5. To enable such students as may find Washington the best point for their special studies, to enjoy the advantages of the Museums, Libraries, Laboratories, Observatory, Meteorological, Piscicultural, and Forestry Schools, and kindred institutions of the several departments of the Government.

6. To ensure the prompt publication and distribution of the results of scientific investigation, a field considered highly important.

If in any year the full income of the Trust cannot be usefully expended or devoted to the purposes herein enumerated, the Committee may pay such sums as they think fit into a Reserve Fund, to be ultimately applied to those purposes, or to the construction of such buildings as it may be found necessary to erect in Washington.

The specific objects named are considered most important in our day, but the Trustees shall have full power, by a majority of two-thirds of their number, to modify the conditions and regulations under which the funds may be dispensed, so as to secure that these shall always be applied in the manner best adapted to the changed conditions of the time; provided always that any modifications shall be in accordance with the purposes of the donor, as expressed in the Trust, and that the Revenues be applied to objects kindred to those named,—the chief purpose of the Founder being to secure if possible for the United States of America leadership in the domain of discovery and the utilization of new forces for the benefit of man.

IN WITNESS WHEREOF, I have subscribed these presents, consisting of what is printed or typewritten on this and the preceding seven pages, on [twenty-eighth] day of [January], Nineteen Hundred and Two, before these witnesses.

ANDREW CARNEGIE.

JANUARY 28th, 1902.

Witnesses,

LOUISE WHITFIELD CARNEGIE,

ESTELLE WHITFIELD.

REMARKS BY MR. CARNEGIE ON PRESENTING HIS TRUST DEED.

Mr. Chairman and Members of the Board of Trustees:

I beg first to thank you for so promptly and so cordially coming forward to aid me in this work by the acceptance of trusteeship. The President of the United States writes me in a note of congratulation "I congratulate you especially upon the character, the extraordinarily high character, of the trustees." Those are his words. I believe that that estimate has been generally approved throughout the wide boundaries of our country.

May I say to you that my first idea while I dwelt upon the subject during the summer in Scotland was that it might be reserved for me to fulfil one of Washington's dearest wishes—to establish a university in Washington. I gave it careful study when I returned and was forced to the conclusion that if he were with us here today his finely balanced judgment would decide that such, under present conditions, would not be the best use of wealth. It was a tempting point suggested to me by the president of the women's George Washington Memorial Association, that the George Washington Memorial University, founded by Andrew Carnegie, would link my name with Washington. Well, perhaps that might justify such association with Washington, and perhaps it is reserved for some other man in the future to win that unique place; because if we continue to increase in population as we have done it is not an improbability that it may become a wise step to fulfil Washington's wish. But while that may justify the association of any other name with his, which is a matter of doubt, still I am very certain nothing else would. A suggestion that this gift of mine, which has its own field, which has nothing to do with the University, except as an aid to one if it is established, which has a field of its own, that is entitled to the great name of Washington, is one which I never for a moment could consider. If the coming university under the control of the Nation—as Washington suggested a national institution—is to be established, as it may be in the future, I think the name of Washington should be reserved for that and for that alone. Be it our opportunity in our day and generation to do what we can to extend the boundaries of human knowledge by utilizing existing institutions.

This is intended to coöperate with all existing institutions because one of the objections—the most serious one, which I could not overcome when I was desirous to establish a university here to carry out Washington's idea—was this: That it might tend to weaken existing institutions, while my desire was to coöperate with all kindred institutions, and to establish what would be a source of strength to all of them and not of weakness, and therefore I abandoned the idea of a Washington University or anything of a memorial character.

Gentlemen, a university worthy of Washington, or a memorial worthy of Washington, is not one costing a million dollars, or ten million dollars, or twenty million dollars, but of more. When I contemplated a university in Washington in fulfilment of Washington's great wish I set a larger amount than the largest of these. I take it for granted that no one or no association would think of using the revered name of Washington except for a university of first class rank, something greater and better, if I may be allowed to say so, than we have in our land today—and you all know the sums which are now used for our universities.

Gentlemen, your work now begins, your aims are high, you seek to expand known forces, to discover and utilize unknown forces for the benefit of man. Than this there can scarcely be a greater work. I wish you abundant success, and I venture to prophesy that through your efforts, in coöperation with kindred organizations, our country's contributions through research and the higher science in the domain of which we are now so woefully deficient, will compare in the near future not unfavorably with those of any other land.

Again, gentlemen, from my heart, I thank you, and I will now, with your permission read the deed of trust which has been prepared. I may say that the intended officers of this Institution have a letter from my cashier, stating that the notice of the transfer of the bonds will be sent you early in February. They cannot be transferred until the first of the month. They begin to bear interest on the first day of February. Here is the deed of trust. *

There is nothing so important, I think, as the last clause. This clause follows the deed given to the Scotch universities, in the main. When I proposed it to the committee the chairman said he did not know about assuming so much responsibility as a trustee, and sev-

* Printed on pages xi–xiv.

eral gentlemen also suggested that it was too liberal, and threw too much responsibility upon them. Mr. Arthur Balfour was one of these. I replied to him that my experience was that it is not without the greatest difficulty we find men who can legislate for their own generation, and sometimes we are not quite successful even in that; but, I asked, "Have you ever seen, or heard of a body of men wise enough to legislate for the next generation?" He answered "No, I never have"; and "You are quite right; that is the wisest provision I have ever heard of in a trust deed."

I have nothing more to say to you, gentlemen, having already expressed my thanks; but, as I began with doing this, I feel that I should also like to end doing so, and therefore, I thank you again.

MINUTES OF MEETING OF INCORPORATORS OF THE CARNEGIE INSTITUTION OF WASHINGTON.

The meeting of the Incorporators of the Carnegie Institution was held at the office of the Secretary of State, Washington, D. C., January 4, 1902, at 10 o'clock a. m.

Present Hon. John Hay, Secretary of State, Justice Edward D. White, Dr. Daniel C. Gilman, Dr. John S. Billings, Hon. Carroll D. Wright, and Dr. Charles D Walcott.

Mr. Hay was chosen Chairman of the meeting, and Mr. Walcott Secretary.

A draft of the proposed Articles of Incorporation was submitted, discussed, and on motion of Mr. White, amended by adding, in the Fourth Article, the words "and shall not thereafter exceed thirty, except by a three-fourths vote of said Board."

The incorporators then signed and acknowledged the Articles of Incorporation.*

The Articles of Incorporation were then taken to the office of the Recorder of Deeds of the District of Columbia by Mr. Marcus Baker, and filed at 11 o'clock a. m. On receipt of notice of the filing of the Articles of Incorporation, Mr. White moved that the incorporators proceed to ballot for trustees. This was done, and the following persons were unanimously elected:

Ex Officio.

THE PRESIDENT OF THE UNITED STATES.
THE PRESIDENT OF THE SENATE.
THE SPEAKER OF THE HOUSE OF REPRESENTATIVES.
THE SECRETARY OF THE SMITHSONIAN INSTITUTION.
THE PRESIDENT OF THE NATIONAL ACADEMY OF SCIENCES.

GROVER CLEVELAND, ...	New Jersey.
JOHN S. BILLINGS, ...	New York.
WILLIAM N. FREW, ...	Pennsylvania.
LYMAN J. GAGE, ...	Illinois.
DANIEL C. GILMAN, ...	Maryland.
JOHN HAY, ...	District of Columbia.

* Printed on pages vii–viii.

ABRAM S. HEWITT,	. . . . . .	New Jersey.
HENRY L. HIGGINSON,	. . . .	Massachusetts.
HENRY HITCHCOCK,	. . . . . .	Missouri.
CHARLES L. HUTCHINSON,	. . .	Illinois.
WILLIAM LINDSAY,	. . . .	Kentucky.
SETH LOW,	. . . .	New York.
WAYNE MACVEAGH,	. . . . . .	Pennsylvania.
D. O. MILLS,	. . . .	New York.
S. WEIR MITCHELL,	. . . . . .	Pennsylvania.
WILLIAM W. MORROW,	. . . .	California.
ELIHU ROOT,	. . . .	New York.
JOHN C. SPOONER,	. . . .	Wisconsin.
CHARLES D. WALCOTT,	. . . .	District of Columbia.
ANDREW D. WHITE,	. . . . . .	New York.
EDWARD D. WHITE,	. . . . . .	Louisiana
CARROLL D. WRIGHT,	. . . . .	District of Columbia.

On motion of Dr. Gilman it was voted to ask Mr. Carnegie whether it would be agreeable to him to call a meeting of the Trustees. He subsequently signified his readiness to meet the Trustees in Washington, January 28, 1902, and the Secretary of State invited them to meet in one of the rooms of the State Department.

On motion of Col. Wright it was voted that the Secretary of the meeting notify the Trustees-elect of their election, and of the time and place of meeting, as approved by Mr. Carnegie, and send a copy of the Articles of Incorporation to each of the Trustees.

At 12.30 the incorporators adjourned.

CHAS. D. WALCOTT,
Secretary of Incorporators.

MINUTES OF FIRST MEETING OF BOARD OF TRUSTEES.

[Abstract.]

The Trustees assembled in the Diplomatic Room, Department of State, Washington, D. C., Wednesday, January 29, 1902, at half past two.

They were called to order by Hon. Abram S. Hewitt, who nominated for temporary chairman Hon. John Hay, who was unanimously elected and took the chair.

Mr. Hewitt then nominated Dr. Charles D. Walcott as temporary secretary, and he was unanimously elected.

The Secretary called the roll and the following Trustees responded to their names:

Ex Officio.

Wm. P. Frye, President of the Senate.
D. B. Henderson, Speaker of the House of Representatives.
S. P. Langley, Secretary of the Smithsonian Institution.

Active.

John S. Billings	C. L. Hutchinson
William N. Frew	William Lindsay
Lyman J. Gage	Wayne MacVeagh
Daniel C. Gilman	D. O. Mills
John Hay	S. Weir Mitchell
Abram S. Hewitt	William W. Morrow
Henry L. Higginson	Elihu Root
Henry Hitchcock	Charles D. Walcott

Carroll D. Wright

Absent: The President of the United States, the President of the National Academy of Sciences, Seth Low, John C. Spooner, Andrew D. White, and Edward D. White.

The Secretary then read the minutes of the meeting of the incorporators and presented the Articles of Incorporation, after which Mr. Andrew Carnegie was introduced by the Chairman, and made the remarks printed on pages xv–xvii.

The following resolution was presented and unanimously adopted:

"In addition to the personal and individual expressions extended to Mr. Carnegie for what he has done for the world today:

"*Resolved*, That the chairman of this meeting be requested to draft a letter addressed to Mr. Carnegie expressing the views of the Trustees concerning this magnificent gift and the purposes for which it is to be applied as set forth in the letter and other documents which have just been read."

The Secretary then read the following letter from the Home Trust Company:

JANUARY 20TH, 1902.

DEAR SIR: I have been instructed by Mr. Carnegie to transfer $10,000,000.00 of United States Steel Corporation 5 per cent bonds to the Trustees of the Carnegie Institution. These bonds will probably be ready for delivery the early part of February, and as soon as they are received from the Transfer Agents I will keep them in a special box in the vaults of the Hudson Trust Company, Hoboken, N. J., subject to the order of the Trustees. Kindly advise if this will be entirely satisfactory to you.

Very truly yours,

R. A. FRANKS.

President,
Trustees of Carnegie Institution,
Washington, D. C.

Attention was called to the vacancy on the Board caused by the declination of Hon Grover Cleveland, who had not found it possible to accept a place on the Board on account of his health.

An extract from the letter of ex-President Cleveland to Mr. Carnegie was ordered inserted in the minutes as follows:

WESTLAND, PRINCETON, N. J., *Jan. 3, 1902.*

MY DEAR MR. CARNEGIE: I have received your letter, tendering me the great honor of a place among the trustees who are to administer your noble benefaction in the cause of the highest education.

After careful consideration I have determined to ask you to allow me to decline your exceedingly flattering proffer.

I hope it is unnecessary for me to assure you that in reaching this conclusion, I have duly weighed every element that enters into the subject; and have thoroughly convinced myself that I ought not to undertake the important duty involved in your invitation.

Yours very sincerely,

GROVER CLEVELAND.

The Board then balloted for a trustee to fill the vacancy thus arising, and Mr. William E. Dodge, of New York, was unanimously elected.

A proposed code of By Laws was then presented, discussed, amended, and adopted. This code, as still further amended at the meeting of November 25, 1902, is printed on pages ix–x.

Election of officers, in accordance with the By Laws just adopted, was then held with the following result:

Chairman of the Board of Trustees, Abram S. Hewitt.

Vice Chairman of the Board of Trustees, John S. Billings.

Secretary of the Board of Trustees, Charles D. Walcott.

President of Carnegie Institution, Daniel C. Gilman.

At the second session of the Board, held on January 30, 1902, the following members were

Present:

WM. P. FRYE, President of the Senate.

S. P. LANGLEY, Secretary of the Smithsonian Institution.

JOHN S. BILLINGS.	WILLIAM LINDSAY.
WILLIAM N. FREW.	WAYNE MACVEAGH.
LYMAN J. GAGE.	S. WEIR MITCHELL.
DANIEL C. GILMAN.	WILLIAM W. MORROW.
ABRAM S. HEWITT.	ELIHU ROOT.
HENRY L. HIGGINSON.	CHARLES D. WALCOTT.
HENRY HITCHCOCK.	EDWARD D. WHITE.
C. L. HUTCHINSON.	CARROLL D. WRIGHT.

Absent:

THE PRESIDENT OF THE UNITED STATES.

D. B. HENDERSON, Speaker of the House of Representatives.

ALEX. AGASSIZ, President of the National Academy of Sciences.

WILLIAM E. DODGE.	D. O. MILLS.
SETH LOW.	JOHN C. SPOONER.

ANDREW D. WHITE.

The minutes of the previous meeting were read and approved.

Relative to the acceptance of the Trust created by Mr. Carnegie, it was—

Resolved: That the Board of Trustees, acknowledging the generosity of the gift of Mr. Carnegie, in the foundation of the Institution, desire to express the concurrence of the Trustees in the scope and purpose stated in his deed of trust, and hereby formally accept the donation and the responsibilities connected with it.

It was also voted that the resolution just adopted be forwarded to

Secretary Hay, to be by him sent to Mr. Carnegie, with a letter expressing the views of the Trustees on the gift.

Mr. Hay subsequently transmitted the resolution and with it the following letter:

DEPARTMENT OF STATE,
WASHINGTON, *March 7, 1902.*

HON. ANDREW CARNEGIE,
5 West 51st Street, New York City.

SIR: The Trustees of the Carnegie Institution, which you have recently founded in the city of Washington, formally accepted your gift, by the adoption of the appended Resolution.

At the same time they requested me, as the presiding officer at the first meeting of the Board, to convey to you by a letter an expression of their hearty appreciation of your munificence, and also their admiration of the noble purpose and the liberal spirit which distinguish your foundation.

For the advancement of knowledge and the education of youth, there are already in this country many strong institutions, learned societies, universities, government bureaus, libraries and museums. With all of them the Carnegie Institution can coöperate, while it has a field of its own, carefully indicated in your deed of gift, and more fully explained by the remarks which you addressed to the Board.

Every one of those whom you have chosen as Trustees will regard it as a sacred duty and a pleasure, to uphold the lofty ideal that you have set before them, and to impart to those who come afterwards the spirit of confidence and enthusiasm with which the work has begun.

I am, Sir,
Very respectfully yours, JOHN HAY.

Dr. Gilman, the elected President, then addressed the Board, explaining, so far as they were known to him, the circumstances which preceded the incorporation of the Carnegie Institution. His remarks were extemporaneous and intended to acquaint the Board with his attitude and that of the gentlemen with whom, at Mr. Carnegie's request, he had been associated in these arrangements which preceded the meeting of the Board. He expressed his appreciation of the honor conferred upon him by his selection as President of the Institution, and he indicated in broad outlines the probable methods of procedure. At an early day experts in many branches of science will be selected by the Executive Committee to whom all applications for encouragement and aid will be referred. These experts will be requested to add their own suggestions, and pre-

sent their recommendations in writing. Meanwhile, the Executive Committee will gather information in respect to endowments and establishments for promoting science, at home and abroad, in order that this experience may be at the service of the Trustees, and that there may be coöperation, and not conflict, with other institutions in any plans that may be adopted.

After discussing nominations the following named persons were elected members of the Executive Committee:

JOHN S. BILLINGS.	ELIHU ROOT.
DANIEL C. GILMAN.	CHARLES D. WALCOTT.
ABRAM S. HEWITT.	CARROLL D. WRIGHT.
S. WEIR MITCHELL.	

The following resolutions were then considered and adopted:

Resolved: That the Executive Committee is requested to prepare a report upon the work which should be undertaken by the Carnegie Institution in the near future, such report to be submitted to the Board of Trustees at its next meeting, and to be accompanied with stimates for expenditures required.

Resolved: That the Executive Committee, when they shall have formulated plans of the work which should be undertaken by the Carnegie Institution, shall have the same printed and a copy forwarded to each Trustee prior to the annual meeting in November, 1902.

Resolved: That the Executive Committee is requested to consider the question of a proper administration building for the Carnegie Institution, to be located in Washington, including both a proper site and plans for the same.

At the same meeting authority to expend $75,000 was granted by the following resolution.

Resolved: That to defray current expenses of administration, as well as special accounts which may be made during 1902, $75,000, or so much thereof as necessary, is hereby appropriated from the income of the Institution, and is placed at the disposal of the Executive Committee.

Resolved: That the United States Trust Company be, and is hereby, designated as a depository of funds belonging to the Carnegie Institution, and that the President of the Institution, jointly with the Secretary of the Board of Trustees, be, and are hereby, authorized to endorse all checks payable to the order of the Carnegie Institution and make checks or drafts against the funds to the credit of the Institution and the depository above designated.

The following order of business was then adopted for future meetings:

1. Reading of Minutes of last meeting.
2. Report of the Secretary.
3. Reports of Committees:
    (*a*) Executive Committee.
    (*b*) Auditing Committee.
    (*c*) Special Committees.
4. Amendments to By Laws.
5. Election of officers.
6. Unfinished business.
7. New business.
8. Adjournment.

Thereupon, at 12.15 P. M., the meeting adjourned.

CHAS. D. WALCOTT,
Secretary.

MINUTES OF SECOND MEETING OF THE BOARD OF TRUSTEES.

[Abstract.]

The meeting was held in Washington, at the New Willard Hotel, on Tuesday, November 25, 1902, at 10 A. M.

In the absence of the Chairman, Mr. Hewitt, the Vice Chairman, Mr. Billings, occupied the chair.

On the roll being called by the Secretary, the following gentlemen responded to their names:

Ex Officio.

WILLIAM P. FRYE, President of the Senate.
D. B. HENDERSON, Speaker of the House of Representatives.
S. P. LANGLEY, Secretary of the Smithsonian Institution.
ALEX. AGASSIZ, President of the National Academy of Sciences.

Active.

JOHN S. BILLINGS.
WILLIAM N. FREW.
LYMAN J. GAGE.
DANIEL C. GILMAN.
JOHN HAY.
HENRY L. HIGGINSON.
C. L. HUTCHINSON.
WILLIAM LINDSAY.
WAYNE MACVEAGH.
D. O. MILLS.
S. WEIR MITCHELL.
WILLIAM W. MORROW.
ELIHU ROOT.
CHARLES D. WALCOTT.
EDWARD D. WHITE.
CARROLL D. WRIGHT.

Absent:

Ex Officio.

THE PRESIDENT OF THE UNITED STATES.

Active.

WILLIAM E. DODGE.
ABRAM S. HEWITT.
SETH LOW.
JOHN C. SPOONER.
ANDREW D. WHITE.

Letters were received from Messrs. Dodge, Hewitt, Low, and White regretting their inability to be present.

The minutes of the first meeting were read and approved.

The President of the Institution, Mr. Gilman, made a general statement of the work of the Executive Committee and referred to

the report of the Committee, which had been printed and distributed to the Trustees in advance of the meeting.

The Secretary made a brief report, referring principally to the financial transactions of the Institution and submitted the following

Financial Statement:

The following is a statement of receipts and disbursements of the Carnegie Institution of Washington from the beginning, in February to October 31, 1902.

All the preliminary expenses of organization, aggregating $1,825.52, were paid by Mr. Carnegie.

The total receipts are	$250,009.70	
The total disbursements are	30,187.48	
Balance on hand October 31, 1902		$219,822.22
Of this balance there is on deposit with the U. S. Trust Company of New York	$209,822.22	
American Security and Trust Company of Washington	10,000.00	
Total		$219,822.22

The receipts were:

August 1.	From the endowment	$250,000.00	
July 3.	From interest on deposits in American Security and Trust Company	9.70	
	Total		$250,009.70

Consideration of the Executive Committee's report was then taken up, and a long discussion followed on the various recommendations made by the Committee.

At 12.15 p. m. the Board took a recess until 2 p. m.

At the second session there were present.

Ex Officio.

WILLIAM P. FRYE, President of the Senate.
D. B. HENDERSON, Speaker of the House of Representatives.
S. P. LANGLEY, Secretary of the Smithsonian Institution.
ALEX. AGASSIZ, President of the National Academy of Sciences.

Active.

JOHN S. BILLINGS.	WILLIAM LINDSAY.
WILLIAM N. FREW.	D. O. MILLS.
LYMAN J. GAGE.	S. WEIR MITCHELL.
DANIEL C. GILMAN.	WILLIAM W. MORROW.
HENRY L. HIGGINSON.	CHARLES D. WALCOTT.
C. L. HUTCHINSON.	EDWARD D. WHITE.

CARROLL D. WRIGHT.

Absent:

Ex Officio.

THE PRESIDENT OF THE UNITED STATES.

Active.

WILLIAM E DODGE	WAYNE MACVEAGH.
JOHN HAY.	ELIHU ROOT.
ABRAM S. HEWITT.	JOHN C. SPOONER.
SETH LOW.	ANDREW D. WHITE.

The Board resumed its discussion of policy and the recommendations of the Executive Committee, especially the purchasing of a site. As the outcome a motion to postpone till the next annual meeting the decision on the question of site was made and carried.

The Board then considered and adopted the following resolution:

Resolved: That from the available income of the Institution $50,000 is hereby appropriated for administrative expenses, $200,000 for grants for research during the fiscal year 1902–'03, $40,000 for a publication fund, the expenditures to be made under the direction of the Executive Committee, and that $100,000 of the available income of the Institution be set apart for a reserve fund during the fiscal year 1902–'03.

Amendments to the By Laws were then considered, and the date of the annual meeting was changed from November to the second Tuesday of December, beginning with the year 1903. By Laws were also adopted providing that the fiscal year of the Institution shall be from November first to October thirty-first, inclusive, and that there shall be a Finance Committee consisting of three members of the Board, to be elected by the Board and to hold office until their successors are elected. The duty of such Finance Committee shall be to consider and recommend to the Board of Trustees such measures as it may believe will promote the financial interests of the Institution.

The By Law in relation to amendments was so modified that thirty days' notice of the proposed amendments must be given prior to the meeting of the Board.

The By Laws as then amended and adopted are printed on pages ix–x.

The Board then proceeded to the choice of the Finance Committee, and elected Messrs. Gage, Mills, and Higginson.

The following minute relative to the death of Mr. Henry Hitchcock was presented by Mr. Higginson and adopted by the Board:

The death of Mr. Henry Hitchcock has deprived this Board of Trustees of a cultured and wise counsellor, a progressive leader, and a valued associate. Mr. Hitchcock stood for all that was noble in manhood and the development of man. His every effort was to serve any cause with which he was connected with all the power and ability he possessed. We tender to the members of his bereaved family sincere sympathy, and place this resolution in our minutes as a permanent record of our appreciation and esteem.

The Board then proceeded to fill the vacancy caused by the death of Mr. Hitchcock. Mr. Ethan Allen Hitchcock was nominated and unanimously elected.

At 4.10 P. M. the Board adjourned.

CHARLES D. WALCOTT,
Secretary.

PROCEEDINGS OF EXECUTIVE COMMITTEE

[Abstract.]

Introduction.—The general powers and duties of the Executive Committee are set forth in the By Laws, pages ix–x; the special duties and powers assigned it by the action of the Trustees at their meeting on January 30, 1902, are comprised in the five resolutions printed on page xxiv. All the proceedings had by the Committee have been pursuant to and in conformity with those powers and instructions.

Meetings.—The Committee held eight meetings, viz:

First meeting, January 30, 1902, in Washington.
Second meeting, February 8, 1902, in New York City.
Third meeting, March 11, 1902 in New York City.
Fourth meeting, March 25, 1902, in New York City.
Fifth meeting, April 15, 1902, in Washington.
Sixth meeting, October 3 and 4, 1902, in New York City.
Seventh meeting, October 27 and 28, 1902, in Washington.
Eighth meeting, November 14, 1902, in Washington.

Organization.—At its first meeting the Committee organized by electing Mr. Gilman Chairman and Mr. Walcott Secretary. At the same time lots were drawn for the terms of service of members, three to expire with the annual meeting in 1903, two in 1904, and two in 1905. The result of the drawing was as follows:

Terms expiring in December,
1903, Gilman, Mitchell, Wright;
1904, Billings, Walcott;
1905, Hewitt, Root.

Seal.—The temporary seal adopted by the Institution contains the words "Carnegie Institution, incorporated January 4, 1902." It is understood that this temporary seal is to be used until a permanent one is adopted by the Trustees.

Advisory Committees.—As soon as it was organized the Executive Committee, in compliance with the instructions of the Trustees, began an investigation to determine what work should be entered upon, in the immediate future, by the Institution. Its first step con-

sisted in the appointment of Advisory Committees. Eighteen such Committees were appointed as follows:

Anthropology:

William H. Holmes, Chief, Bureau of American Ethnology, and Head Curator, Department of Anthropology, U. S. National Museum, Washington, D. C., *Chairman.*

Franz Boas, Curator, Department of Anthropology, American Museum of Natural History, New York, N. Y.

George A. Dorsey, Field Columbian Museum, Chicago, Ill.

Astronomy:

E. C. Pickering, Professor of Astronomy and Director of Harvard Observatory, Cambridge, Mass., *Chairman.*

Lewis Boss, Director of Dudley Observatory, Albany, N. Y.

George E. Hale, Director of Yerkes Observatory, Williams Bay, Wis.

S. P. Langley, Secretary Smithsonian Institution, Washington, D. C.

Simon Newcomb, late Superintendent of Nautical Almanac, Washington, D. C.

Bibliography:

Herbert Putnam, Librarian of Congress, Washington, D. C., *Chairman.*

Cyrus Adler, Librarian, Smithsonian Institution, Washington, D. C.

J. S. Billings, Director New York Public Library, New York, N. Y.

Botany:

Frederick V. Coville, Botanist, Department of Agriculture, Washington, D. C., *Chairman.*

N. L. Britton, Superintendent, New York Botanical Garden, New York, N. Y.

John M. Macfarlane, Professor of Botany, University of Pennsylvania, Philadelphia, Pa.

Gifford Pinchot, Forester, U. S. Department of Agriculture, Washington, D. C.

Chemistry:

Ira Remsen, Professor of Chemistry and President of Johns Hopkins University, Baltimore, Md., *Chairman.*

T. W. Richards, Professor of Chemistry, Harvard University, Cambridge, Mass.

Edgar F. Smith, Professor of Chemistry, University of Pennsylvania, Philadelphia, Pa.

Economics:

Carroll D. Wright, Commissioner of Labor, Washington, D. C., *Chairman.*

Henry W. Farnam, Professor of Political Economy, Yale University, New Haven, Conn.

John B. Clark, Professor of Political Economy, Columbia University, New York, N. Y.

Engineering:

R. H. Thurston, Director of Sibley College, Cornell University, Ithaca, N. Y., *Chairman.*

William H. Burr, Professor of Civil Engineering, Columbia University, New York, N. Y.

George Gibbs, Consulting Engineer, Baldwin Locomotive Works, Philadelphia, Pa.

George S. Morison, Civil Engineer, 49 Wall Street, New York, N. Y.

Charles P. Steinmetz, Electrician, General Electric Co., Schenectady, N. Y.

Geography:

William M. Davis, Professor of Geology, Harvard University, Cambridge, Mass.

Geophysics:

[Joint Committee on Geology and Physics.]

Geology:

T. C. Chamberlin, Head of Geological Department and Director of Museum, University of Chicago, Chicago, Ill., *Chairman.*

Charles R. VanHise, Professor of Geology, University of Wisconsin, Madison, Wis.

Charles D. Walcott, Director of U. S. Geological Survey, Washington, D. C.

History:

J. Franklin Jameson, Head of Department of History, University of Chicago, Chicago, Ill., *Chairman.*

Charles Francis Adams, Boston, Mass.

Andrew C. McLaughlin, Professor of American History, University of Michigan, Ann Arbor, Mich.

Mathematics:

E. H. Moore, Head Professor of Mathematics, University of Chicago, Chicago, Ill., *Chairman.*

Frank Morley, Professor of Mathematics, Johns Hopkins University, Baltimore, Md.

Ormond Stone, Professor of Astronomy and Director of Leander McCormick Observatory, Charlottesville, Va.

Meteorology:

Cleveland Abbe, Professor of Meteorology, U. S. Weather Bureau, Washington, D. C.

Paleontology:

Henry F. Osborn, DaCosta Professor of Zoölogy, Columbia University, New York, N. Y., *Chairman.*

Henry S. Williams, Professor of Geology, Yale University, New Haven, Conn.

Physics:

R. S. Woodward, Dean of School of Pure Science and Professor of Mechanics and Mathematical Physics, Columbia University, New York, N. Y., *Chairman.*

Carl Barus, Professor of Physics, Brown University, Providence, R. I.

A. A. Michelson, Head Professor of Physics, University of Chicago, Chicago, Ill.

Physiology (including Toxicology):

S. Weir Mitchell, Philadelphia, Pa., *Chairman.*

H. P. Bowditch, Professor of Physiology, Harvard Medical School, Cambridge, Mass.

William H. Howell, Dean of Johns Hopkins Medical School, Baltimore, Md.

Psychology:

J. Mark Baldwin, Professor of Psychology, Princeton University, Princeton, N. J.

Zoölogy:

Henry F. Osborn, DaCosta Professor of Zoölogy, Columbia University, New York, N. Y., *Chairman.*

Alex. Agassiz, Curator Natural History Museum, Cambridge, Mass.

W. K. Brooks, Professor of Zoölogy, Johns Hopkins University, Baltimore, Md.
C. Hart Merriam, Chief U. S. Biological Survey, Washington, D. C.
E. B. Wilson, Professor of Zoölogy, Columbia University, New York, N. Y.

These Advisers were requested to give the committee their views on various important suggestions received by the Institution, as well as independent recommenuations originating in the committees. The following is a copy of the letter appointing the Advisers and inviting suggestions and recommendations.

MARCH 11, 1902.

DEAR SIR:

The Executive Committee of the Carnegie Institution have been requested by the Trustees to prepare, in the course of the summer, a plan of procedure, and in the meantime to engage in preliminary studies of the problems committed to them, by consultation with acknowledged authorities at home and abroad.

The plan of the Institution includes the appointment from time to time of counsellors, or advisers, to whom the Committee may refer important suggestions, and from whom they may receive independent recommendations. You are invited to act as one of these advisers until the annual meeting of the Trustees, in November next. It is the purpose of the Institution to provide liberally for any expense that may be incurred in clerical service and in travel by those whom they may consult. If it is agreeable to you to accept this invitation, a more personal communication will be addressed to you at an early day. An immediate answer is requested.

Respectfully,

D. C. GILMAN,
President.

The reports received from the Advisory Committees, as far as they relate to scope and plan, are printed in Appendix A.

Circular Letter.

A circular letter was also prepared and sent to nearly a thousand scientific men and investigators of prominence, mainly in the United States. This was accompanied by a pamphlet that included the articles of incorporation, the founder's address, and a list of the officers. The circular letter is as follows:

Letter to the Heads of American Institutions and to Others Interested in the Work of Investigation.

The Carnegie Institution sends to you herewith a copy of Mr. Carnegie's deed of gift and other information in respect to the organization of the new foundation.

Some of the ablest thinkers and investigators in the country have already called attention to important lines of inquiry. Their communications will be referred to special committees in different departments of knowledge—astronomical. physical, chemical, biological, geological, archæological, philolcgical, historical, bibliographical, economical, etc.—and the referees will be requested to add their own suggestions and to report to the Carnegie Institution such methods of procedure and the names of such investigators as they deem likely to advance with wisdom the great purpose of the foundation.

No large appropriations can be made at present, as there will be no income from the fund before August. The summer will be chiefly devoted to a careful study of the problems of scientific investigation, at home and abroad, and in the autumn definite plans of procedure will be formulated.

Any member of the Executive Committee will be glad to receive from you at any time suggestions, opinions, and advice as to fields that the Carnegie Institution ought to occupy and the best methods for carrying forward its work in those fields; but in order that important papers designed for official consideration may be properly recorded and filed, they should be addressed to the President of the Carnegie Institution, 1439 K street, Washington, D. C.

DANIEL C. GILMAN, *Chairman*,
CHARLES D. WALCOTT, *Secretary*,
JOHN S. BILLINGS,
ABRAM S. HEWITT,
S. WEIR MITCHELL,
ELIHU ROOT,
CARROLL D. WRIGHT,
Executive Committee.

MARCH, 1902.

For its guidance, the Committee has formulated and adopted the following statements as to its *Purposes*, *Principles*, *Organization*, and *Policy* :

Purposes.—In connection with the determination of the policy of the Institution, it is necessary to clearly define its *purposes* and to adopt some general plan for organization and administration. The purposes, are declared by the Founder to be

"To found in the city of Washington an institution which, with the coöperation of institutions now or hereafter established, there or

elsewhere, shall in the broadest and most liberal manner encourage investigation, research, and discovery—show the application of knowledge to the improvement of mankind, provide such buildings, laboratories, books, and apparatus as may be needed, and afford instruction of an advanced character to students properly qualified to profit thereby."

And he adds:

"That his chief purpose is to secure, if possible, for the United States of America, leadership in the domain of discovery and the utilization of new forces for the benefit of man."

The trust deed enumerates several aims, all of which may be grouped under two heads, viz:

(A) To promote original research.

(B) To increase facilities for higher education.

Under (A) may be grouped:

(*a*) The promotion of original research "as one of the most important of all subjects."

(*b*) To discover the exceptional man * * * and enable him to make the work for which he seems specially designed his life work.

(*c*) The prompt publication and distribution of the results of scientific investigation.

Under (B) may be grouped:

(*a* and *b*) The increase of facilities for higher education by increasing the efficiency of the universities and other institutions, either by utilizing and adding to their existing facilities or by aiding teachers in various institutions in experimental and other work.

(*c*) To enable such students as may find Washington the best point for their special studies to take advantage of the facilities there for higher education and research.

Principles.—It is the judgment of the Executive Committee that the aims enumerated can be best carried into effect under the following principles, which are to be departed from only in very exceptional cases.

The Institution proposes to undertake—

(A) To promote original research by systematically sustaining—

(*a*) Projects of broad scope that may lead to the discovery and utilization of new forces for the benefit of man, pursuing each with the greatest possible thoroughness.

(*b*) Projects of minor scope that may fill in gaps in knowledge of particular things or restricted fields of research.

(*c*) Administration of a definite or stated research under a single direction by competent individuals.

(*d*) Appointment of Research Assistants.

(B) To increase facilities for higher education by promoting—

(*a*) Original research in universities and institutions of learning by such means as may be practicable and advisable.

(*b*) The use by advanced students of the opportunities offered for special study and research by the Government bureaus in Washington.

The Institution does not proposes to undertake—

(*a*) To do anything that is being well done by other agencies.

(*b*) To do that which can be better done by other agencies.

(*c*) To enter the field of existing organizations that are properly equipped or are likely to be so equipped.

(*d*) To give aid to individuals or other organizations in order to relieve them of financial responsibilities which they are able to carry, or in order that they may divert funds to other purposes.

(*e*) To enter the field of applied science except in unusual cases.

(*f*) To purchase land or erect buildings for any organization.

(*g*) To aid institutions when it is practicable to accomplish the same result by aiding individuals who may or may not be connected with institutions.

(*h*) To provide for a general or liberal course of education.

Organization.—The Executive Committee, keenly realizing the importance of thoroughly investigating and fully considering every proposed action before recommending it to the Trustees, have given much time and thought to the subject of *organization*, and at the several meetings have discussed the suggestions received from individuals and from the Advisory Committees. It is hoped and expected that the Institution will set a high standard for research, This the Committee believes can be best attained and maintained by establishing such laboratories and facilities, not found elsewhere. as are necessary when dealing with great problems.

The Committee is of the opinion that *organization in Washington* should be provided for by—

(*a*) Purchasing in the northwestern suburb of the city a tract of ground suitable for present and future needs.

(*b*) Erecting thereon a central administration building, to serve as the administrative headquarters of research work conducted, directed, or aided by the Carnegie Institution.

(*c*) Establishing such laboratories from time to time as may be deemed advisable.

(*d*) Employing the best qualified men that can be secured for carrying on such research work as it may be decided to undertake in Washington.

(*e*) Continuing and developing the present office organization as the Executive Committee may find it necessary to do in order to properly conduct the work of the Institution.

The only *organization outside of Washington* to be provided for at present should be such advisers and advisory committees as may from time to time be found necessary in connection with the development of the research work of the Institution. It is the opinion of the Committee that such persons and committees should be largely advisory and not executive in their function. Executive work should be in charge of paid employés of the Institution. These may be officers, research associates, and special employés.

Policy.—Soon after the Executive Committee began its investigations it became evident that two lines of policy were open, namely:

(*a*) To sustain broad researches and extended explorations that will greatly add to knowledge.

(*b*) To make small grants.

Research may be defined as original investigation in any field, whether in science, literature, or art. Its limits coincide with the limits of the knowable. In the field of research the function of the Institution should be organization, the substitution of organized for unorganized effort wherever such combination of effort promises the best results; and the prevention, as far as possible, of needless duplication of work. Hitherto, with few exceptions, research has been a matter of individual enterprise, each worker taking up the special problem which chance or taste led him to and treating it in his own way. No investigator, working single handed, can at present approach the largest problems in the broadest way thoroughly and systematically.

With an income large enough to enter upon some large projects and a number of minor ones, it appears to be wiser, at the beginning, to make a number of small grants and to thoroughly prepare to take up some of the larger projects. With this in view the Exec-

utive Committee recommended to the Trustees that there be placed at its disposal for the *fiscal year 1902–'03*, two hundred thousand dollars for aid to special researches in various branches of science, and $40,000 for the publication of the results achieved. During the year plans will be perfected, data secured, and experience gained that will be of great service in formulating recommendations for the ensuing year.

In the opinion of the Committee, the most effective way to discover and develop the exceptional man is to put promising men upon research work under proper guidance and supervision. Those who do not fulfil their promise will soon drop out, and by the survival of the fittest the exceptionally capable man will appear and be given opportunity to accomplish the best that is in him. When the genius is discovered, provide him with the best equipment that can be obtained.

In making grants the wisest policy appears to be to make them to individuals for a specific purpose rather than to institutions for general purposes.

Grants.—Under the authority conferred upon it by the Trustees at their first meeting, the Executive Committee made three grants, as follows:

March 25, 1902.	To the Marine Biological Laboratory, Woods Hole, Mass., for general support	$4,000
April 15, 1902.	To Dr. J. McK. Cattell, Columbia University, New York, for preparing a list of the scientific men of the United States	1,000
April 15, 1902.	To Dr. Hideyo Noguchi and Professor Simon Flexner Philadelphia, Pa., for continuation of their studies of the toxicological actions of snake venom and allied poisons	1,000
Total		$6,000

Since the second meeting of the Trustees, on November 25, 1902, the Executive Committee has made the following grants in the several departments of science mentioned; anthropology, mathematics and other branches will be acted upon later:

Astronomy	$21,000
Bibliography	15,000
Botany	11,700
Chemistry	3,000
Economics	15,000
Engineering	4,500

Exploration	$5,000
Geology	12,000
Geophysics	8,500
History	5,000
Investigation of project for southern and solar observatory	5,000
Investigation of project for physical and geophysical laboratories	5,000
Investigation of natural history projects	5,000
Marine biological research	12,500
Paleontology	1,900
Physics	4,000
Physiology	5,000
Psychology	1,600
Publications	5,500
Research assistants	25,000
Student research work in Washington	10,000
Zoölogy	4,000
Total	$185,200

CHARLES D. WALCOTT,
Secretary.

SUMMARY

As a convenient summary of the plans and methods thus far agreed upon the following minute is approved :

The methods of administration of the Carnegie Institution thus far developed are general rather than specific.

The encouragement of any branch of science comes within the possible scope of this foundation, but as the fund, munificent as it is, is inadequate to meet the requests for aid already presented, not to mention others which are foreseen though not yet formulated, attention has been concentrated upon a selection of those objects which, at this time and in our country, seem to require immediate assistance.

Efforts have been and will be made to secure coöperation with other agencies established for the advancement of knowledge, while care will be exercised to refrain from interference or rivalry with them. Accordingly, ground already occupied will be avoided. For example, if medical research is provided for by other agencies, as it appears to be, the Carnegie Institution will not enter that field. Systematic education, abundantly provided for in this country by universities, colleges, professional schools, and schools of technology, will not be undertaken. Nor will the assistance of meritorious students in the early stages of their studies come within the scope of this foundation. Sites or buildings for other institutions will not be provided.

Specific grants have been and will be made, for definite purposes, to individual investigators, young or old, of marked ability, and for assistance, books, instruments, apparatus. and materials. It is understood that such purchases are the property of the Carnegie Institution and subject to its control. The persons thus aided will be expected to report upon the methods followed and the results obtained. In the publication of results it is expected that the writer will say that he was aided by the Carnegie Institution of Washington, unless it be requested that this fact be not made known.

In order to carry out the Founder's instructions in respect to bringing to Washington highly qualified persons who wish to profit by the opportunities for observation and research afforded by the various scientific bureaus of the United States Government, a certain sum is set apart for this purpose.

In addition, the Carnegie Institution will appoint from time to ime a number of persons to be known as Research Assistants, who may or may not reside in Washington, and who shall undertake to carry on such special investigations as may be entrusted to them by the Institution. The appointments will be made for a year, and may be renewed in any case where it seems desirable. Permission may be given to go abroad, if special advantages not accessible in this country can thus be secured.

Publication is regarded by the Founder as of special importance. Accordingly, appropriations will be made for this purpose, especially for the printing of papers of acknowledged importance, so abstruse, so extended, or so costly that without the aid of this fund they may not see the light.

With respect to certain large undertakings involving much expense, which have been or may be suggested, careful preliminary inquiries have been and will be made.

In order to secure the counsel of experts in various departments of knowledge, special advisers have been and will be invited from time to time for consultation. Valuable suggestions and counsel have already been received from such advisers.

DANIEL C. GILMAN,
President of the Carnegie Institution.

WASHINGTON, *November 25, 1902.*

MEMORIAL.

HENRY HITCHCOCK.

1829–1902.

At the annual meeting of the Trustees of the Carnegie Institution held November 25, 1902, the death of Mr. Henry Hitchcock, one of the Trustees, having been announced, it was ordered, on motion of Mr. Higginson, that the following minute be adopted and spread upon the record:

The death of Mr. Henry Hitchcock has deprived this Board of Trustees of a cultured and wise counsellor, a progressive leader, and a valued associate. Mr. Hitchcock stood for all that was noble in manhood and the development of man. His every effort was to serve any cause with which he was connected with all the power and ability he possessed. We tender to the members of his bereaved family sincere sympathy, and place this resolution in our minutes as a permanent record of our appreciation and esteem.

The following account is copied from the Obituary record of graduates of Yale University, June, 1902, No. 61, pp. 135–138:

Henry Hitchcock, son of Hon. Henry Hitchcock (University of Vermont, 1813) and Anne (Erwin) Hitchcock, was born on July 3, 1829, at Spring Hill, six miles from Mobile, Ala. His father was a native of Burlington, Vt., Secretary of the Territory of Alabama, Attorney General and afterward Chief Justice of the State of Alabama, a man of the highest character, beloved throughout the State; and his grandfather, Samuel Hitchcock (Harvard, 1777), who married a daughter of Ethan Allen, was United States District and Circuit Judge, drafted the charter of the University of Vermont, was Secretary of the same from 1790 to 1800, and Trustee from its beginning until his death in 1813. His mother was the daughter of Colonel Andrew Erwin, of Bedford county, Tennessee.

After the death of his father his mother removed with her family first to Kentucky, and then to Nashville, Tenn. There he entered the junior class in the University of Nashville, and received the degree of Bachelor of Arts in November, 1846. Immediately afterward he came to New Haven and joined the class, then in its junior year, in Yale College, and graduated with the honor of an oration.

From August to November, 1848, he was a law student in the office of Hon. Willis Hall, Corporation Counsel of New York city, and was then assistant classical teacher in the Worcester, Mass., High School for a year, after which he returned to Nashville and continued his legal studies in the office of Hon. William F. Cooper, later a Justice of the Supreme Court of Tennessee. In September, 1851, he settled permanently in St. Louis, Mo., was admitted to the bar in October, and began practice. During the year 1852 he was assistant editor of the *St. Louis Intelligencer*, and represented that paper at the National Whig Convention in Baltimore, but afterward devoted himself entirely to the practice of his profession.

In 1872 he formed a partnership with George W. Lubke and John Preston Player, and the firm of Hitchcock, Lubke and Player thus formed, continued until 1882, when Mr. Lubke was elected a Judge of the Circuit Court, soon after which Mr. Player died.

Mr. Hitchcock then practiced alone for two years, and in 1884 formed a limited partnership with Judge George A. Madill and Hon. Gustavus A. Finkelnburg, which expired in 1890. He continued with the latter until July, 1891, and afterward again practiced alone. He devoted himself especially to equity, corporation and constitutional law.

For over forty years he was deeply interested in Washington University, St. Louis, of which he became a director in 1859, and vice president in 1886. In August, 1867, he helped organize its law department, known as the St. Louis Law School, and for the first twelve years was dean. He was also professor of various departments of law until his retirement, in 1884.

After Lincoln's debate with Douglas on the Kansas-Nebraska question, he joined the Republican party and became an active opponent of slavery. In January, 1861, he was elected a member on the "Unconditional Union" ticket of the Missouri State Convention, which was called by the Secession Legislature to consider the relations of Missouri to the Union, but which disappointed expectations, and deposed both governor and legislature, and for more than two years carried on a provisional state government. He took an active part in its proceedings and attended all its sessions until its final adjournment, on July 1, 1863.

He had earnestly desired active service in the war, and as soon as the Union interests in his own State permitted, he entered the army and was appointed Assistant Adjutant General, U. S. Volunteers, and

from October 1, 1864, to the close of the war served as Judge Advocate on the personal staff of General Sherman. He was with the latter on the "March to the Sea" and in the subsequent campaign through the Carolinas, and carried to Washington the dispatches announcing the "Sherman-Johnston truce." He was brevetted Lieutenant-Colonel, and honorably mustered out of service on June 23, 1865.

After the war he spent four months in European travel. Five years later, owing to the failure of his health, he made a voyage to visit his brother, Ethan Allen Hitchcock, who was then engaged in business in Hongkong, China, and is at present Secretary of the Interior.

In August, 1871, he was one of the delegates who organized at Newport, R. I., the National Civil Service Reform League, and from that date was a member of its Executive Committee. He was one of the fourteen signers of the call which resulted in the formation, in August, 1878, at Saratoga, N. Y., of the American Bar Association, and served several years on standing and special committees, notably on the Committee on the Relief of the United States Supreme Court. He prepared the majority report advocating the plan afterward substantially followed by Congress in creating United States Circuit Courts of Appeal. He was elected President of the Association in 1889.

In 1880 he helped organize the Missouri State Bar Association, of which he was President in 1881.

In April, 1896, he was a delegate from Missouri to the American Conference on International Arbitration, held at Washington, D. C., and took part in its debates, earnestly advocating an international arbitration treaty with England.

He delivered addresses on various subjects of professional and public interest, including the annual address before the New York State Bar Association in January, 1887, on "American State Constitutions," afterwards published in the series called "Questions of the Day;" the annual address before the American Bar Association the same year on "General Corporation Laws;" in March, 1889, an address before the Political Science Association of the University of Michigan, on the "Development of the Constitution as Influenced by Chief Justice Marshall," which, with other lectures by well-known lawyers, was published in a volume entitled "Constitutional Law;" and at the centennial celebration of the organization of the

Supreme Court of the United States, in New York, in February, 1890, an address on "The Exercise of the Powers of the Court," a historical review of the principal decisions on constitutional questions. He received the degree of Doctor of Laws from Yale College in 1874.

Since the establishment of the Missouri Botanical Garden at St. Louis, by bequest of Mr. Henry Shaw, in 1889, he had been Vice-President of the Board of Trustees.

Mr. Hitchcock died at his home in St. Louis, after an illness of several weeks, from heart disease, on March 18, 1902, in his seventy-third year.

He married, on March 5, 1857, Mary, eldest daughter of George Collier, a prominent merchant of St. Louis, and had two sons, who, with their mother, survive.

APPENDIX A

REPORTS OF ADVISORY COMMITTEES

REPORT OF ADVISORY COMMITTEE ON ECONOMICS

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: The committee appointed to report upon plans for economic research are of the opinion that, among the numerous topics in Economics, Sociology, and Public Law which might be interesting and useful to study, none are at the present time more promising than those which are suggested by the economic and legislative experience of our states. This experience presents such diversities and the matter to be studied is so vast that it is almost impossible for an isolated investigator to deal thoroughly with even a very limited phase of it. The government offices are obviously not in a position to treat it with the freedom demanded by science. The Carnegie Institution is, therefore, both on account of the funds at its command and on account of its power of enlisting the coöperation of scholars throughout the country, in a position of peculiar advantage with respect to this kind of work, and is able to direct a series of investigations of inestimable value, which, but for its assistance, might not be undertaken for many years.

Among the many topics which fall within this general field, we may specify:

(1) Social legislation of the states, which should be critically examined with reference to its results.

(2) The labor movement.

(3) The industrial development of the states.

(4) State and local taxation and finance.

(5) The state regulation of corporations.

The thorough, scientific presentation of these and other allied topics would constitute a monumental economic history of the United States and occupy a place in economic literature hitherto unfilled.

The committee recommend that an appropriation of $15,000 a year be made provisionally for a period of five years, believing that during that time valuable results can be produced; they further recommend that the committee be authorized to carry forward such parts of the work as may be most advantageously undertaken, and to employ such assistance as may be necessary.

Respectfully submitted by

CARROLL D. WRIGHT, *Chairman*,
HENRY W. FARNAM,
JOHN B. CLARK,
Committee.

NEW YORK, *April 9, 1902*.

REPORT OF ADVISORY COMMITTEE ON BOTANY

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: Your Advisory Committee on Botany have the honor to propose the following

PLAN FOR BOTANICAL RESEARCH.

CONTENTS.

OUTLINE OF PLAN.

I. Relation of Vegetation to Environment in the United States.

A. The function and effect of the forest with regard to atmospheric moisture, precipitation, and run-off, and the converse effect on the forest.

B. The establishment and maintenance of a desert botanical laboratory for the purpose of ascertaining the methods by which plants perform their functions under the extraordinary conditions existing in deserts.

II. Botanical Explorations and Researches in Central America and the West Indies.

A. An exploration of these regions with reference to the composition of the different floras, their relationship to each other and to the climate and geological formations with which each is associated, and their interrelationship with the aboriginal or native peoples, bearing on the past influence of the vegetation on the races of mankind and the probable future development of the various areas.

B. An extensive system of collecting and determining species and recording data, as a means of obtaining the facts necessary to a proper understanding of the problems outlined above, and for other purposes.

C. The establishment and maintenance, in the region, of a botanical station, laboratory, and garden for the solution of problems requiring such equipment

Explanation of Plan.

I. Relation of Vegetation to Environment in the United States.

A. *The function and effect of the forest with regard to atmospheric moisture, precipitation, and run-off, and the converse effect on the forest.*

The relation of forests to atmospheric moisture, precipitation, and run-off is one of the largest economic problems of forestry. It is directly concerned with the water supply for dry and irrigation farming, for water power, and for navigation, and with the control of floods. Although it is the text for continual discussion, we do not yet know whether forests influence rainfall or not, or what is the exact relation of the forest to run-off.

More than seventy thousand square miles of national forest reserves have been created, chiefly with a view to the protection of the water supply in the arid and semi-arid west, without adequate scientific demonstration of their effectiveness. It may be noted also that the flood loss along streams which rise in the Southern Appalachian Mountains, during the last twelve months, was upwards of eighteen million dollars. The lack of precise information on a topic so fundamentally important requires to be remedied, and is capable of remedy. The failure of previous investigations has had much to do with the neglect of a technical knowledge of the nature of the forest and of the differences between forest types. The opportunity to reach results of great value is an admirable one. These results will not be reached, at least for many years, by any agency other than the Carnegie Institution, since no other is capable of coöperating at the same time with all the indispensable members of this inquiry. A suitably conducted study will substantially solve this problem, it will not be undertaken unless by the Institution, and it is thoroughly worthy of investigation along the following general lines:

The investigation should be in charge of a trained forester, assisted by a meteorologist, a hydrographer, a plant physiologist, and an authority on plant distribution, each to be of the first rank. A Greek and Latin scholar, with some knowledge of the general subject under investigation, should be employed during one year to examine and sift the historical evidence for climatic change following deforestation.

A digest of meteorological observations and measurements of run-off already at hand is to be studied in relation to forest types, first in the United States and later abroad (notably in British India and the West Indies), if necessary. Simultaneously the historical study above mentioned should proceed, with special reference to Mediterranean countries and British India. Large coöperation may be expected from the Weather Bureau, the Geological Survey, the Bureau of Forestry, and other government organizations. Field study of forest types and rainfall and forest types and run-off, both on restricted areas, would be required, but such studies should take place usually in regions of sudden changes of forest and climate, and should not restrict the general breadth of the investigation, which should be distinctively continental in character.

B. *The establishment and maintenance of a desert botanical laboratory for the purpose of ascertaining the methods by which plants perform their functions under the extraordinary conditions existing in deserts.*

There should be established at some point in the desert region of the southwestern United States a laboratory for the study of the life history of plants under desert conditions, with special reference to the absorption, storage, and transpiration of water. Although there are many botanical laboratories in the humid portions of the temperate regions, as well as several marine laboratories and tropical laboratories devoted in whole or in part to botanical research, a desert botanical laboratory exists nowhere in the world. Yet the phenomena presented in the adaptations of plants to desert conditions are among the most interesting and significant, from an evolutionary point of view, of any in the whole realm of botany.

The economic ground for the establishment of such a laboratory is the enormous development of population and industries that is bound to take place in our arid region during the next hundred years. The basis of that development is agriculture, both with and without irrigation. At the present time comparatively little is known about the peculiar fundamental processes of plant growth under the unusual conditions surrounding plant life in that region. The investigations proposed are of so general a character, so expensive, and so difficult that no agricultural experiment station has as yet undertaken them, and there is no prospect that any station will do so. When, however, the processes of plant growth in our deserts have been thoroughly investigated and are well understood, the botanists of the agricultural experiment stations in the arid states will be in a position to make a practical application of this knowledge to the special agricultural crops of the region.

It would be necessary to provide a building with equipment of apparatus and a reference library, the whole to be in charge of a resident investigator. It would be desirable also to furnish working quarters for about five additional investigators, some of whom undoubtedly would be the botanists of experiment stations or other botanists giving themselves special training for such positions. As the work of the laboratory progressed it might be found wise to make special grants of money to one or more of these additional investigators. The initial period of maintenance should be not less than five years.

II. Botanical Explorations and Researches in Central America and the West Indies.

A. *An exploration of these regions with reference to the composition of the different floras, their relationship to each other and to the climate and geological formations with which each is associated, and their interrelationship with the aboriginal or native peoples, bearing on the past influence of the vegetation on the races of mankind and the probable future development of the various areas.*

One result of the Spanish-American War has been to open to the botanists of the United States a new field of research, that of tropical botany. The floras of Central America and the West Indies offer an accessible, rich, and comparatively unworked field. At the same time the recent discovery of the manner in which the germs of yellow fever and malaria are transmitted has removed the chief obstacle to the penetration of the white race into the tropics. A revolution in the methods of tropical agriculture is a probable, almost inevitable, result of American influence. This has already occurred in the case of cane sugar, the principal tropical crop with which Americans have had to do, and great changes have occurred in the culture of coffee, rice, and bananas as a result of partial influence from the same source There is every reason to believe that the coming century will see as great a transformation in tropical agriculture as that which has been brought about in America in the past century in temperate agriculture.

The natural vegetation of any area is a resultant of its climatic, topographic, and geological conditions, and the vegetation of adjacent districts varies as these conditions vary. We know in a general way that the low malarial regions of the Central American coasts are covered with a certain type of dense forest. We know that the interior plateaus and mountains have a wholly different type of forest, or no forest at all, and are often healthy places well suited for

the development of population and industries. We need, however, authoritative and precise information about each of these floral areas, its location, boundaries, characteristic vegetation, the interrelation of its component elements to each other, the characteristic features of the climate with which each flora is associated, and the relation of the vegetation to geological formations.

Finally, each flora should be investigated from the standpoint of its interrelationship with the people who are associated with it geographically. A study of the plants used by any aboriginal race, particularly a prehistoric race, throws great light on its industries, migrations, and civilization. The Central American tropics furnish a wealth of material of this sort which has never been exploited from a critical botanical standpoint.

The geographic botanical investigation here outlined promises results of great significance in ethnologic science, while the clear knowledge of the vegetative covering of the region which is contemplated will be fundamentally useful in the social and industrial development of these areas which is plainly foreshadowed by the commercial necessities of the United States.

B. *An extensive system of collecting and determining species and recording data as a means of obtaining the facts necessary to a proper understanding of the problems outlined above, and for other purposes.*

In addition to the necessity of extensive collecting in order to secure the facts necessary in the geographic researches above described, there are other reasons why it is desirable to increase our knowledge of the plants of the West Indies and Central America. These regions have been partially explored from time to time, but continuous systematic examination of them has not yet been possible, and it is certain that a large number of plant species new to science. and of others about which little is known, occur there. Every botanical collector who visits any part of the region brings back previously unknown forms. The field is therefore a most fertile one for taxonomic research.

Many species whose life history has never been studied occur in the West Indies and Central America, and their investigation would certainly shed much light on questions of development and relationship.

Problems in comparative morphology and anatomy almost without number remain unsolved among species existing only in the American tropics, and the geographic distribution of the species comprising the tropical flora can only be made better known by further exploration.

The New York Botanical Garden, by means of a bequest by the late ex-Chief Justice Charles P. Daly and other funds at its command, has undertaken, under the direction of Prof. L. M. Underwood and Prof. N. L. Britton, the preparation and publication of a Systematic Botany of North America, designed to furnish citations and descriptions of all plant species occurring in North America, including subarctic regions, the West Indies, and Central America and comprising all plants from the simplest to the most complex, from the Myxomycetes to the Compositae. Coöperation with the botanists of other institutions has been arranged for in this work.

The further exploration of the North American tropics would render this work the more complete, and therefore the more valuable. Several trained botanical collectors should be put in the field, the material obtained by them to be used in the preparation of the Systematic Botany. The presence of trained collectors in the region would afford opportunity for securing material of particular species needed by investigators in various laboratories for the solution of problems under study.

The economic features of the investigation would certainly be valuable; the uses of tropical plants are multifarious, and many new applications of them and their products in the arts and manufactures would be likely to ensue; the horticultural aspects of the study would also be important.

C. *The establishment and maintenance, in the region, of a botanical station, laboratory, and garden for the solution of problems requiring such equipment.*

The present status of the morphology and physiology of plants rests chiefly upon observations made on species indigenous to a region about fifteen degrees in width extending across Europe and America in the north temperate zone, together with some information derived from the study of a comparatively few species from other parts of the world, which could be cultivated in the region in question under more or less abnormal conditions.

It has been made evident many times that generalizations reached by such limited and circumscribed methods are unsafe and misleading, and it has become plainly apparent that the entire subject of botany may be developed in a manner commensurate with its scientific and economic importance only when a systematic effort is made to extend investigations to cover the vegetation of the tropics, in which many of the more important physiological and morphological

types are abundant, exhibiting a range of adaptation and a luxuriance of development unknown in the temperate regions.

The desirability and necessity for such an extension of botanical research has long been recognized, yet but little progress has been made in securing adequate facilities in the matter. The Dutch government maintains a research laboratory in pure botany in connection with its great plantations and collections at Buitenzorg, Java, and the small number of botanists who have been able to undertake the long journey to this laboratory have achieved results which fully justify the above estimate of the value of such an institution. The government of India is devoting some attention to similar developments in Ceylon and in one or two other places. An effort was made to organize a tropical laboratory in the West Indies a few years since, but the movement was interrupted by the Spanish-American War, and it will not be possible to resume the plan for organization except by such aid as might be given by the Carnegie Institution.

The establishment of a botanical station of the intended scope and functions would not only afford opportunities for the furtherance of research in all of the strictly technical aspects of the science, but the results obtained would include much of economic importance at a time when it seems necessary for all tropical American countries to improve their methods, or modify the character of their agricultural operations. A station of the above kind would be easily accessible to all botanical investigators in America, and might, if properly located, become the foremost tropical laboratory in the world, since any part of tropical America is also capable of being reached quickly by European students.

The material equipment of the proposed station would consist of a suitable building of metal or stone sufficient to accommodate a scientific staff of two or three persons and provide additional space for at least a dozen investigators. The building should be furnished with the necessary apparatus and appliances for microscopical and experimental research and a small working collection of books. Stress is to be laid on the fact that the best method of management of such stations consists not in acquiring an extensive outfit for the purpose of anticipating all of the wants of the workers who may visit it, but in furnishing the elementary essentials of a station as a beginning and then maintaining the ability to meet the particular needs of the individual investigator. The station should have under its control a few acres of ground in which cultural tests, operations, and

experiments may be carried on, and it should be provided with means to make limited explorations in the adjacent country to secure material needed by its investigators.

A tropical station should reach its highest efficiency when placed under the direction of a competent botanist, who should become a resident in the vicinity of the laboratory. The director should have as an assistant a person trained in the methods of laboratory practice and whose most important duty would consist in securing living material for the investigators who might visit the station. The proper maintenance of the building would require the services of a keeper or superintendent who should control the work of the laborers and gardeners in the care of the building.

The laboratory should be organized for a period of five years. With the information at hand accumulated by the former tropical laboratory commission, the director could proceed at once to the selection of a site and the erection of a suitable building. This work and the arrangement of the equipment would consume the greater part of a year, during which period the director only need be employed, the remainder of the staff beginning their duties at the close of the first year. The assistant to the director might, however, profitably begin his duties at any time convenient in the establishment of the station.

ESTIMATE OF COST.

RELATION OF FORESTS TO CLIMATE.

Forester in charge	$1,500	
Traveling and other expenses	1,500	
		$3,000
Historical study, one year only		1,500
Collaborating meteorologist—report, research, and advice		1,000
Collaborating hydrographer—report, research, and advice		1,000
Collaboration in plant physiology and geographic distribution		1,000
		$7,500
First year, $7,500	$7,500	
Four subsequent years at $6,000	24,000	
Total for five years	$31,500	

DESERT LABORATORY.

Equipment:		
Building	$2,000	
Apparatus	2,000	
Books	1,000	
		$5,000

Maintenance for five years:		
First year	$3,000	
Second year	3,250	
Third year	3,500	
Fourth year	3,750	
Fifth year	4,000	
		$17,500
Total for five years		$22,500

TROPICAL PLANT GEOGRAPHY.

Working force:		
Botanist (six months)	$1,500	
Botanical assistants (three)	4,200	
Geological assistant	2,000	
Bibliographical assistant	720	
Photographer	1,000	
		$9,420
Traveling expenses		4,000
Supplies and miscellaneous expenses		2,580
Total for the year		$16,000

COLLECTING TROPICAL PLANTS.

Salaries of three experienced tropical collectors for six months, at $125 per month each	$2,250
Expenses of these collectors at $150 per month each	2,700
Contingent fund	50
Total for the year	$5,000

TROPICAL LABORATORY.

Expenditures during the first year of organization:		
Building	$3,000	
Equipment	2,000	
Salary of director	2,500	
Expenses of director in selecting site	500	
		$8,000
Maintenance during second year:		
Salary of director	$3,000	
Salary of assistant	1,500	
Salary of keeper	1,000	
Gardeners and laborers	1,000	
Additions to equipment, repairs, etc.	1,000	
		$7,500
Maintenance during third year		8,000
Maintenance during fourth year		8,000
Maintenance during fifth year		8,000
Total cost of establishment and maintenance of tropical laboratory and station during five years		$39,500

SUMMARY OF FIRST YEAR'S COST.

Relation of Forests to Climate	$7,500
Desert Laboratory	8,000
Tropical Plant Geography	16,000
Collecting Tropical Plants	5,000
Tropical Laboratory	8,000
Total	$44,500

ESTIMATED YEARLY AVERAGE FOR FIVE YEARS.

(Each investigation should be continued for not less than five years.)

Relation of Forests to Climate	$6,300
Desert Laboratory	4,500
Tropical Plant Geography	16,000
Collecting Tropical Plants	5,000
Tropical Laboratory	7,900
Total yearly average	$39,700

These estimates do not include the publication of results.

* * * * * * *

Respectfully submitted by

FREDERICK V. COVILLE, *Chairman*,
N. L. BRITTON,
JOHN M. MACFARLANE,
GIFFORD PINCHOT,
Committee.

WASHINGTON, *June 28, 1902.*

REPORT OF ADVISORY COMMITTEE ON PHYSICS

CONTENTS.

To the Executive Committee of the Carnegie Institution:

GENTLEMEN: Your Committee appointed to suggest ways and means for the increase and utilization of knowledge in the domain of Physics beg to submit the following report:

INTRODUCTION.

By common consent the domain of Physics is divided into several fields, which, though overlapping to some extent, are sufficiently distinct to justify separate consideration. These fields are those of Heat, Light, Electricity, Sound, and Mechanics, by which latter all of the others are to a greater or less extent correlated.

Great advances have been made in recent times, especially during the last half century, in all of these subjects. One needs only to recall the astonishing progress in the practical applications of physi-

cal science in our own time to realize this fact. The developments of heat engines, photography, electricity, telephony, telegraphy, etc., familiar to everybody, afford sufficient justification if any is needed from the utilitarian point of view, for the promotion of physical research. But surprising and gratifying as have been these advances, and well worked as have been all of the fields referred to, there is the amplest room for improvement in every one of them and promise of still more important practical developments from investigations in most of them.

In the past, many capital discoveries and many wide generalizations have been made by isolated workers, mostly college or university professors, who have pursued research more or less incidentally in connection with their duties of instruction. Often such discoveries have required only meagre instrumental equipment and mediocre intellectual attainments, for the phenomena to be observed lay close at hand. With the advance of science, however, it is becoming continually essential to attack larger and more difficult problems. In general, the more recondite the phenomena to be investigated the more elaborate must be both the instrumental and the intellectual equipment of the investigator; and the more essential is it that he should devote his entire time and energy, rather than a fraction of them, to his work of research. There are some physical questions, also, like those common to astronomy and geodesy, which require for their solution the cooperation of a number of experts through a series of years. Such cooperation in the great national and private observatories and in national and international geodetic bureaus has hitherto secured results of immense theoretical and practical importance. Similarly, the Royal Institution of London has been made famous by the researches of Davy, Faraday, Tyndall, and Dewar.

It is clear, then, that there are at the present epoch unlimited opportunities for physical research; and that we may confidentially expect a rich harvest of results from almost any field diligently cultivated by one or more competent investigators. In conformity with this view, your Committee is of opinion that the Carnegie Institution by promoting research in the more refined and difficult problems of physics may not only advance knowledge directly through important resulting discoveries, but may also advance knowledge indirectly by so distinct a recognition of the value of abstract investigation.

Your Committee believe it best to consider all privileges and appointments as emanating from the Institution and none as being obtainable on solicitation from without. Vacancies which may be filled by the best applicant who happens to present himself, should not exist. The full justification for such honours as may be conferred, should be found in the testimony of the annals of physical science. In recommending the following course of action, the Committee, therefore, beg leave to state at the outset, that it is not their design to have all the privileges to be hereinafter mentioned, conferred either continuously or at once; that it would rather be their policy to find the physicists first, and then to build the facilities around them. Furthermore, they have, throughout their work, proceeded from the conviction that as it is the purpose of a university to develop the untried investigator, so it should be the function of the Carnegie Institution to begin with the physicist whose development is acknowledged; that it should extend to him its unstinted opportunities, in proportion as his powers of research are keener and more mature.

The following specific recommendations for effectively encouraging research in pure physics in the United States, are given first in outline, secondly in detail, and supplemented by a provisional plan.

II. Recommendations in Outline.

The recommendations of your Committee are:

(1) *Laboratory.*—To establish a well equipped physical laboratory, to be devoted to research in pure physics, exclusively, and particularly to investigations which, from their character, from the lack of apparatus necessary, etc., can not be advantageeusly done elsewhere. The completion of the laboratory is to be approached gradually by appropriating a definite fund each year, which fund shall be used as a minimum permanent endowment forever after. The resources which so accumulate will in due time suffice for the construction of buildings, for equipment, salaries and the general maintenance of the said laboratory, in the way hereinafter to be more definitely specified. An annuity of 150,000 dollars is estimated as sufficient for this purpose.

(2) *Temporary Associates.*—To secure the co-operation with the Carnegie Institution, of physicists of recognized eminence, who are already provided with working facilities at other institutions of learning or elsewhere, by an annual grant for a specified term of years, in recognition of their continuous contributions to the scien-

tific transactions of the Carnegie Institution. Twenty grants of 2000 dollars each, to be designated *temporary non-resident research associateships*, or a total of 40,000 dollars per annum, is deemed sufficient for this purpose.

(3) *Permanent Associates.* To establish a few small laboratories in different parts of the country, placed in charge of persons specially eminent for their exceptional experimental skill in the direction of discovery in pure physics and who will not profit by the recommendations under (1) and (2). The advance of physics depends so much on the intellectual effort of individuals, many of whom can not do their best work under corporate surroundings, that this recognition of the habits of work of persons whose scientific record is beyond question, has seemed to the Committee a provision of importance. Frequently, moreover, the character of the work will demand that it be done in special localities. The physicists here in question are not to lose financially by being transferred to the Carnegie Institution; consesequently the following estimates of the grants, to be known as *permanent non-resident research associateships* are given as an average case; viz., for laboratory and grounds, 10,000 dollars; for salary, 6,000 dollars; for running expenses, equipment and assistants, 4,000 dollars. The Committee recommend that two such private laboratories be open for establishment at once, to be increased to four in the course of time, as the funds become available. The total annual expenditure is not to exceed 40,000 dollars.

(4) *Grants.* To establish a fund for grants given to persons of recognized ability, or to societies, for apparatus, laboratory equipment, clerking, shopwork, publication, etc. The Committee have learned that this need has already been partially met by other endowments, and that a wiser provision for the material status of the individual investigator is the more important demand at the present time. They therefore deem an annuity of 10,000 dollars sufficient, under existing circumstances, for grants of the kind specified.

(5) *Council.* To retain a Council of not less than three nor more than five physicists of the highest eminence, whose duty it shall be to inaugurate the research facilities specified and to watch over their progress. A fee of 2,000 dollars per annum to each member is deemed sufficient for the services asked.

(6) *Summary of Annuities.* The total annual appropriation recommended by your Committee for research in pure physics stands, therefore, as follows:

Annuity for a physical laboratory, etc.,	$150,000
for permanent research associates with small laboratories,	40,000
for temporary research associates,	40,000
for grants of apparatus, etc.,	10,000
for the Council,	10,000
Total,	$250,000

III. Recommendations in Detail.

The Committee further beg leave to submit the following more detailed outline of the laboratory, its government, personnel, functions, equipment and maintenance, together with a final table in which the appropriation recommended is differently itemized.

(A) Laboratory, Resident Officers and Council.

(7) *Laboratory.* The Council proposes that a laboratory, devoted to the higher order of physical research, be built in some specially favorable locality. The work to be prosecuted in this laboratory is to be purely investigational in the broadest sense, to the exclusion of mere routine work of any kind.

The cost of the laboratory is estimated at $400,000; of the grounds, whose location shall be sufficiently isolated to meet the requirements of modern research, at $50,000; of the Power house, shop, etc., at $50,000; making a total of $500,000; the equipment is estimated at $100,000, and the running expenses at $30,000 per annum, or an average of 5,000 dollars for each of the departments hereinafter specified.

(8) *Physicists in charge.* The "physicists in charge" of the work are to be, without exception, persons of internationally acknowledged eminence in physics, and whose records are accessible in the annals of science. No other voucher shall be considered. If the person needed is not to be found in the United States, he may be appointed from abroad, but no position need be filled continuously.

The salaries of the physicists in charge shall be a minimum of $6,000 per annum. If called from other institutions, they shall be appointed at a salary not less than that received immediately prior to their appointment.

They are to be appointed by the Trustees, upon recommendation of the Council on physics. The recommendation is to be made with

the scientific history of the candidate attached, and the appointment is to be for life, subject to the approval of the Council.

After the age of 62 years, or in case of disability, the physicist in charge is at liberty to become inactive and to ask for a pension.

The physicists in charge shall be appointed as follows:

(1) A physicist in charge of the department of mechanics.

(2) A physicist in charge of the department of experimental molecular physics, including elastics, capillarity, cohesion, diffusion, solution, crystallization, hydromechanics, viscosity, etc.

(3) A physicist in charge of the department of heat, including thermodynamics, thermal analytics, kinetic theory etc.

(4) A physicist in charge of the department of sound.

(5) A physicist in charge of the department of light and radiation, including geometric optics, spectrum analysis, photography, interference diffraction, polarization, absorption, dispersion, etc.

(6) A physicist in charge of the department of electricity, including electrostatics, magnetism, electromagnetism, electric current, electrolysis, thermoelectrics, induction, electric oscillation, electro-radiation, etc.

Each department is to be filled only when the efficient man has been found. It need not be filled continuously.

The physicist in charge shall be the controlling officer in each department, and the *resident research associates* and *research assistants* hereinafter to be specified shall be subject to his jurisdiction.

It shall not be necessary for the physicist in charge to devote himself exclusively to the subjects of his department, but a wise and watchful provision for its broadest interests is to be his paramount duty.

(9) *Director*. From among these chiefs of division, one shall be selected by the Council, as Director, who shall have the control of all immediate questions of laboratory government.

Appeals from the decision of the Director may be referred to the Council.

The total salary of the Director shall be not less than $8,000 per annum.

(10) *Executive Officer*. To relieve the Director of the burden of administrative work. there shall be appointed an executive officer subject to the control of the Director, at a salary of not less than $4,000 per

annum. It shall be the primary function of this officer to attend to the financial and other administrative work connected with the laboratory. He is to be a man of scientific attainments and to be nominated by the Director. He shall not be eligible to the Council. This executive officer shall have three or more assistants: as, for instance, an electrician, a foreman, laborers, etc., at an aggregate salary of $5,000, to be appointed by himself.

The executive officer shall assist the Council, whenever called upon, in its administrative work.

(11) *Council.* The general supervision of the work, and the decision on all points beyond the jurisdiction of the Director and vitally controlling the progress of the laboratory shall rest with the Council on physics. This Council of not less than three shall be appointed by the Trustees, and shall consist of persons of the same international distinction already specified. They are to be scientists living in the atmosphere of physical research, acquainted with and fruitfully contributing to contemporaneous scientific investigation in physics.

The members of the Council shall be appointed for a period of six years, after which they may be reappointed. The candidates for non-resident councillorships shall be the non-resident research associates hereinafter specified.

One Councillor shall be nominated every two years, beginning with 1904, by the following institutions or societies and in the order specified: the nomination in 1904 is to be by the physicists in charge and the permanent non-resident associates of the Carnegie Institution; the nomination in 1906 by the pure physicists of the National Academy of Sciences of the United States; the nomination in 1908 by the Physical Society of America; the nomination in 1910 by the physicists in charge and permanent non-resident associates of the Carnegie Institution; and so on in rotation.

The Council shall be empowered to increase its number by nominating not more than two (2) physicists of the grade of eminence specified, and each such appointment shall be for a period of six (6) years.

Vacancies are to be filled by the institution or society which nominated the councillor whose place has become vacant.

But one of the Council shall be a resident of the Laboratory; preferably the Director.

The salary of the members of the Council not otherwise connected with the laboratory shall be $2,000 per annum, together with necessary travelling and clerical expenses.

The non-resident Councillors shall be citizens of the United States, and shall not come from the same institution of learning.

The Council through the executive officer, shall solicit a library of reprints of the original work done in pure physics, in the United States or elsewhere, by Americans, in order to remain additionally informed as to the status of the physicists of the country, in regard to their efficiency as investigators.

After continuous service of more than twelve (12) years, a member of the Council who has passed sixty two (62) years of age may become inactive and ask for a pension of his full salary of $2,000.

Three members of the Council shall constitute a quorum, and a majority of votes shall decide.

(B) Resident and Non-resident Associates and Assistants.

(12) *Permanent non-resident associates.*—In recognition of the peculiar habits of work of great investigators, and for other reasons, there shall be established not more than four *permanent non-resident associateships of research.* They shall be recommended to the Trustees by the Council. A specific account of the accomplishments of the candidate is to be attached to the recommendation.

These permanent non-resident associateships shall be granted to persons of the recognized exceptional eminence, herein already specified, in recognition of their past and present continuous services to scientific research in physics. If appointed from another institution, it shall be at a salary not less than that received immediately prior to the appointment and not less than 6,000 dollars per annum. They shall be provided with a laboratory of their own design, costing not more than $10,000 for grounds and building, and not more than $4,000 per annum for assistants, equipment and maintenance.

These associateships shall be filled only when the person of recognized ability has been found, and they need not be filled continuously.

(13) *Temporary non-resident associates.*—To bring the laboratory in touch with the wishes of the Founder as expressed in his deed of trust: to provide that the Institution may react usefully on the country at large, with a view to stimulating research; and in order to reciprocally stimulate those resident at the physical laboratory, there shall be established not more than twenty *temporary non-resident associateships of research.* They shall be recommended to the Trustees by the Council, after consultation with the physicist in

charge in whose department the candidate is eminent. The recommendation shall be accompanied by the scientific history of the candidate.

These associateships shall be granted to men of the recognized exceptional eminence already specified, connected with other institutions of learning, in consideration of their past and present continuous services to scientific research in physics.

It is hoped that in the interest of the advance of science, the person in question will be found deserving of additional encouragement on the part of the university or college to which he belongs, which encouragement may be an increase in salary, or relief from excessive teaching, or from routine duties, or any other reciprocal courtesy. It is to be specifically understood that the associateship is to be an *addition* to whatever material rank the recipient may have previously attained at his own institution.

The associateships shall be filled only when the person of the recognized ability has been found. They need not be filled continuously.

The temporary associateships shall consist of a grant of two thousand dollars per annum and be subject to renewal every five years, and after five renewals they shall be permanent and constitute a pension.

In return for the honor extended to the associate by the Carnegie Institution, the Council will ask for no further service than a connected annual account from the recipient, of the work on which during the term of his appointment he has been engaged. Each such account shall be given with the necessary scientific rigor and they shall together constitute his qualification for reëlection.

(14) *Resident associates.* In addition to the non-resident associateships, there shall be not more than fifteen resident associateships established at an average salary of $1,000 per annum. These associateships will probably fall to the lot of those showing most ability among the resident assistants hereinafter specified. The appointment shall be made for three years, after which it may be renewed.

The resident associates shall be nominated by the Council, after consultation with the physicist in charge in whose department the assistant has worked, and the Council shall state the scientific accomplishments of the candidate.

(15) *Research assistants.*—In order further to encourage research throughout the country, it is recommended that the Council be

empowered, after consultation with the physicists in charge, to nominate not more than twenty research assistantships open to young men of promise.

The recipients must have at least attained the equivalent of the highest degree at an institution recognized by the Council; they must have done a high character of original work, and be properly endorsed.

The research assistants shall be at liberty to pursue their investigations, whether theoretical or otherwise, under the supervision of the Council, either at the physical laboratory of the Carnegie Institution or elsewhere.

The salary of the research assistants shall be an average grant of $1,500 per annum, and the appointment shall be for two years, after which it may be renewed.

Endorsements of research assistants shall come from the physicists in charge, the temporary and permanent research associates, resident and non-resident.

(C) Grants.

The sum of 10,000 dollars is annually to be set apart to be expended at the discretion of the Council in the form of smaller grants, distributed to aid the advance of promising researches in physics.

The applicant for a grant must be at least of the grade of experience of a *research assistant;* he must be well endorsed, and his project clearly stated.

The Council shall not pledge itself to recommend an annual grant for an indefinite period, unless the application comes from a physical society of national standing, and it shall then use its discretion.

A scientific account of the work done with the aid of a grant shall be submitted to the Institution for publication, and the Council shall be empowered to call for such an account.

(D) Tabulated Summary.

An itemized statement of the costs of the laboratory, personnel, equipment, etc., is given in the following table:

Laboratory.	Physical Laboratory	$400,000
	Grounds	50,000
	Power house and shops, etc.	50,000
	Total	**$500,000**
	Original equipment	**$100,000**

This amount is to be collected gradually out of the annuity of $150,000 specified above. With the laboratory, etc., constructed, the annuity is to be expended as follows:

RESIDENT APPOINTMENTS.	Salaries of: Director	$8 000
	Physicists in charge 5 @ $6000	30,000
	Executive officer	4,000
	Aids, foreman, electrician, laborers, etc	5,000
	Resident research associates, 10 @ $3,000	30,000
	Resident research assistants, 20 @ $1,500	30,000
	Instrument makers, etc	2.000
	Janitors and carpenters	2,000
	Clerks and computers	3,000
	Total annual running expenses and additions to equipment	30,000
	Unforeseen expenses	6,000
	Total	**$150,000**

To this is to be added the expense incurred outside of the laboratory, as follows:

NON-RESIDENT APPOINTMENTS.	Annuities for: Permanent non-resident associateships, with assistants and laboratory expenses, 4 at $10,000 each	$40,000
	Temporary non-resident associateships, 20 at $2,000 each	40,000
	Councillors, 5 at $2,000	10,000
	Grants to aid research	10.000
	Total	**$100,000**

IV. PROVISIONAL PLAN.

To inaugurate the project which your Committee has just explained, it will be necessary to appoint a Council possessing the qualifications and entrusted with the duties above set forth. The fees of each member are to be $2,000 per annum.

With regard to the laboratory, your Committee think it prudent to begin with not more than one department. This it seems to them should be the department of light and radiation, since it is in the domain of optics that American research has been peculiarly fruitful and advanced, and a larger number of sufficiently able investigators is most likely to be available. The annual appropriation of $150,000 specified above, should be used to appoint the physicist in charge, the executive officer and his aids, and two research associates in residence; the remainder, which will considerably exceed $100,000, should be devoted to building purposes. The cost of the laboratory

for optical research is estimated at $125,000, and its equipment at $30,000. When the laboratory is sufficiently advanced and equipped, a third research associate and six research assistants in residence, together with instrument makers, clerks, computers, etc., should be appointed.

With regard to appointments not in residence, your Committee after prolonged and careful consideration, with testimony, recommend that in view of the opportunities of the laboratory, but one permanent research associateship and seven temporary research associateships be established at once. This implies an outlay of $34,000.

Finally the Committee are of the opinion that $10,000 should be made available for grants.

The total expenditure for the first year would therefore be $200,000.

* * * * * * *

Respectfully submitted

R. S. WOODWARD, *Chairman.*
CARL BARUS.
A. A. MICHELSON

Committee.

SEPTEMBER 26, 1902.

REPORT OF ADVISORY COMMITTEE ON GEOLOGY

To the Board of Managers of the Carnegie Institution.

GENTLEMEN: Your Advisory Committee on Geology is of the opinion—

(*a*) That research in Geology is fairly well provided for by existing agencies.

(*b*) That the fundamental problems involving research in which Geology, Physics, and Chemistry must be more or less combined should be referred to the Advisory Committee on Geophysics.

(*c*) That certain special investigations and explorations in little known areas should be taken up by the Carnegie Institution.

In conformity with the above, the grants shown in the accompanying exhibit* are recommended for the fiscal year 1903.

Respectfully submitted by

T. C. CHAMBERLIN, *Chairman*,
CHAS. R. VAN HISE,
CHAS. D. WALCOTT,
Committee.

* Not here printed.

REPORT OF ADVISORY COMMITTEE ON GEOPHYSICS

CONTENTS.

I. Introductory Statement.

To the Board of Trustees of the Carnegie Institution.

Gentlemen: Your Advisory Committee to whom were referred the several questions relative to researches in geophysics, have had the same under very careful and prolonged deliberation, and submit herewith their conclusions. The Committee on Physics and Geology have jointly considered all matters relative to geophysical researches, while the Committee on Geology has considered questions essentially geological, and the deliberations of the Committee on Physics have been wholly independent. The joint deliberations of the committees on the actual problems presented have developed the fact that many of the geological and geophysical problems blend into each other so intimately as to offer no good cleavage line for practical separation, while the researches in pure physics are definitely separable from geophysics. Our final conclusion is that the researches in geophysics should be intimately associated with those in geology in administration, while those in physics should be administered independently.

The attention of the committee was largely occupied in considering the most advantageous method of providing for the experimentation which is indispensable to the progress of the profounder studies of earth problems.

Until recently the natural sciences and physical sciences have been handled as if almost independent of each other. The ground between has been largely neglected. The occupancy of this ground is certain to lead to important results. The order of results to be expected is illustrated by the great advances which have recently come from occupying the middle ground between astronomy and physics, and between physics and chemistry. For a long time astronomy and physics were pursued as independent sciences. The recent great discoveries of astro-physics have shown the advantages of their combination. Chemistry and physics for a long time were pursued as independent sciences. The rapid rise of physical chemistry has shown how wonderfully fruitful is the ground between the two.

No individual, university, or State has attempted to study in a comprehensive way the great territory between geology, physics and chemistry. Nor, so far as we know, does an individual, university, or State contemplate the attempt. This great, almost untouched territory, can only be occupied by geophysical laboratories properly equipped and manned.

Your committees have therefore given prolonged consideration to the following propositions:

(1) That a central laboratory of geophysics be established at Washington;

(2) That from this central laboratory the co-operation be sought of all independent laboratories engaged in geophysical studies, without reference to country;

(3) That, where necessary, branch laboratories be constructed in various parts of the world.

The thought has thus been to provide for geophysical work otherwise neglected, and to bring all the work done on this line by the Carnegie Institution under a unified and harmonious system.

(*a*) *The need for laboratories of geophysics.* Relative to the need to be supplied, we beg to reiterate that there are nowhere laboratories that are at all commensurate with the problems to be undertaken, nor is there any reasonable expectation that any established institution will undertake the great task of founding and maintaining such laboratories. This arises from the exceptional nature of investigations applicable to so great and so complex a body as the earth. The problems of geophysics and geochemistry involve the applications of pure physics and pure chemistry from the minutest parts of the earth to the mass of the earth as a whole, and even to

other celestial bodies. The earth presents the grandest concrete example, and the grandest congeries of concrete examples, of the properties of matter available to us. The phenomena presented by the earth are the historical products of chemical and physical forces. From the phenomena we infer the nature of the chemical and physical forces which produced them. However, these inferences require confirmation or refutation by special chemical and physical tests. Usually the phenomena are complex, and it is impracticable to decide between a number of hypotheses as to the manner in which the chemical and physical forces have accomplished the results. In such cases experimental work is absolutely essential in order to make correct inferences as to the nature and manner of work of the forces which have produced the phenomena observed Moreover, the history of the earth, in large part well preserved in the rocks, is undoubtedly typical of the history of the other planets of the solar system, and hence affords a key to the problems of stellar systems in general.

Geophysics, using the term broadly to include geochemistry and related sciences, is founded on pure physics and chemistry ; its data are supplied chiefly by geology ; and the ramifications of its superstructure extend into astronomy and astrophysics.

The principles of physics have been demonstrated experimentally for only a very limited range of conditions. The experiments of physical laboratories usually relate to small masses only, under ordinary pressures and moderate temperatures, with brief time limits. The problems of the earth involve immense masses, extraordinary pressures, very high temperatures and enormous lapses of time. Before the results of ordinary experiments can be safely applied to the great earth problems, they must be tested for the most extreme ranges of mass, pressure, temperature and time that can be commanded. It is quite certain that nearly all the deductions from experiments under common surface conditions are subject to modification under the extraordinary conditions of the earth's interior, and the nature of these modifications must be determined before safe application of the deductions can be made. It is obvious that the determinations reached by such geophisical researches will be contributions of a high order of value to the science of physics itself.

(*b*) *The province of laboratories of geophysics.* There are three great fields which should be embraced within the researches of the laboratory. These blend into each other and embrace many minor fields and the researches in each will contribute to those of the oth-

ers, while the questions in each will stimulate and react helpfully upon one another. These are (1) *the great envelopes of the earth*, the atmosphere and the hydrosphere, which constitute the chief sources of external activities and condition the habitability of the globe; (2) *the body of the earth*, whose crust records its history, and whose interior is full of dark, intricate problems: and (3) *the motions and external relations of the earth*, which condition its form, its external temperature, its illumination, and probably its magnetism and other important phenomena. These lie in the common ground of several sciences, notably in the over-lapping border-fields of physics, chemistry geology and astronomy, but they present special problems whose central interest is terrestrial.

The only hope of adequate solution of these profound problems lies in special experimentation. Neither the application of existing science, with its limitations, nor of pure theory, with its present narrow basis, covers, in any competent way, the ground of these problems. Their magnitude and complexity are such as to require a degree of extension of deductions quite beyond the limits warranted by present experimental data. It is therefore necessary that special experiments shall be devised to determine with the utmost precision the laws of variation, with varying conditions, under as great a range of mass, pressure, temperature and time as possible, and then to apply these determinations with the most critical circumspection to the phenomena which the earth itself presents, and to check them by cross-investigation based upon independent sources. The soul of the method should be to determine by the grandest and at the same time the most refined artificial experiments what is the meaning of the magnificent experiments expressed in the evolution of the earth. The experimentation should therefore be shaped with special reference to the phenomena of the earth, and be checked by all the cross-lines of evidence that can be drawn from geological and astronomical data.

(*c*) *Some of the problems to be investigated.* It is quite beyond the scope of this report to set forth adequately the particular questions to be investigated, but the mention of some of the more declared ones may serve to give definiteness to the undertaking recommended.

1. The envelopes of the earth doubtless in large measure had a common origin, and they are still most intimately related in action and function. In a narrow sense the atmosphere is the domain of meteorology, but its greater problems reach back into the domains

of geology, geophysics and astronomy. Among its sailent questions are those of its origin, its mass, its mass-limitations, its mass-distribution, the potential atmosphere absorbed in the ocean and in the body of the earth, its sources of depletion and enrichment, the constancy or fluctuation of its volume and of its constituents, its function as a thermal blanket, the possible changes in its diathermacy, and the relations of these to great climatic chances, together with many related problems that enter profoundly into the interpretations of the earth's past, and seem to have immense importance to the future of our race.

In these problems of the atmosphere, the hydrosphere participates. It also presents questions of its own as to its origin, mass and mass-distribution; the constancy or variation of the volume of the ocean; the possible function of the waters in promoting their own seggregation and the relation of this to the extension and withdrawal of the ocean relative to the land; the part which the water-mass plays in the changes of form of the earth; the origin, constancy, or variation of the ocean's salinity, the significance of this in past and future history, and many other questions.

The solution of nearly all of these problems rests ultimately upon grounds that need the searching tests of the laboratory. For example, a rigorous determination of the diathermacy of the air, and of its dependence on the several atmospheric constituents, and on their relations to each other, their ionization, their nucleation and their other states, is just now most urgently needed. So is also a critical inquiry into the part the ocean is competent to play in absorbing and giving forth atmospheric constituents under different conditions of temperature, pressure, tension, salinity and biological content.

2. The crust of the lithosphere has thus far been the chief field of geology in the narrower sense, since it contains the rock record of the earth's past, and geological studies have peen directed chiefly to reading and mapping this record. But the record needs to be interpreted on broader and deeper lines, based on a profounder knowledge of physical laws. To this end the data of geology need to be correlated and unified under these laws on an experimental basis. Apart from the record in the layers of sediment, there is a recondite but less legible record in the dynamic features of the earth, and to read these successfully will require the utmost resources of research, involving the fullest available aid of geodetic, physico-chemical and mathematico-physical investigation.

Some of the salient problems of the outer lithosphere are the origin and maintenance of the continental platforms—with their superposed mountains and plateaus—and the abyssmal basins, involving questions of rigidity, isostasy, etc.; the agencies and conditions that make possible the prolonged periods of crustal quiescence shown in baseleveling, and the antithetical epochs of crustal disturbance; the sources of crustal displacement and distortion, shown intensely in the faulting and in the crumpling of mountains and plateaus, and shown massively in the continents and oceanic basins; the mashing, shearing, and foliation of the rocks leading on to the general problems of metamorphism, and a whole group of intricate questions of a chemical and chemico-physical nature, including the flow of rocks, the destruction and genesis of minerals, the functions of included water and gases, the internal transfer of material, the origin of ore deposits, the evolution and absorption of heat, and other phenomena that involve the effects of temperature, pressure, tension, and resultant distortion upon chemical changes and mineralogical aggregations.

These questions of the earth's outer part are inseparably bound up with those of the interior, and here the problems involve the most extreme and the least known conditions, and make their strongest demand for experimental light. The themes here are the kinds and distribution of the lithic and metallic materials in the deep interior: the states of the matter; the distribution of mass and of density, and the consequent distribution of pressure; the origin and distribution of heat; the conductivities of the interior material under the pressure and heat to which it is subjected; the heat possibilities arising from supposed original gaseous condensation, or alternately from initial impact of aggregation; the heat of subsequent attractional condensation; the secular redistribution of heat within the earth, and its loss from the surface; the possible relations of redistribution of internal heat to vulcanism and to deformation, and similar profound problems.

A series of specific laboratory questions arise from these, e. g., the effect of pressure on the melting point of rocks carried to as high temperatures and pressures, and through as wide ranges of material as possible, to develop the laws of constancy or of variation; the effect of temperature and pressure on thermal conductivity as indicated above, and on elasticity, especially as involved in the transmission of seismic tremors.

One of the most hopeful resources for disclosing the constitution and conditions of the interior is found in the transmission of earthquake vibrations directly through the body of the earth, such as are now being recorded in different parts of the globe. These disclose extraordinary elasticity in the center of the earth but before this fact can be safely interpreted the effects of pressure and heat on the elasticity of different classes of earth material in its different states need to be determined with the greatest practicable precision for the greatest practicable range of pressures, temperatures, materials and states of materials.

As a factor in the great problem of interior heat, the compressibility of rock material, the amounts of heat developed in compression and recrystallization need careful determination, and, if possible, also the question whether compressibility has definite limitations or not.

Intimately related to the problems outlined above, and to a great extent doubtless entangled with them, are the problems of physical geodesy and terrestrial magnetism. Important light on the distribution of mass in the crust and nucleus of the earth must be derived from an extended gravimetric survey of the earth's surface. The various national geodetic surveys may be depended on to furnish an increasing amount of data in gravimetric measures, but it ought to be one of the functions of a geophysical laboratory to assist actively in such work, which is largely incidental to the operations of geodesy.

Similarly the problem of terrestrial magnetism belongs rather with geophysics than geodesy. It appears to be indeed, in some of its aspects, a cosmological question, and should be studied as an important problem in itself rather than as one incident to geodetic and topographic work.

3. The relation of the earth to neighboring bodies, its motions and the modification of its form imposed by these, also constitute a record of the earth's history, and a forecast of its future, since the organization of a planet or a planetary system is as much the record of past events as the organization of a rock or a system of sediments, but it is extremely difficult to read. It can only be deciphered positively when the sign manual of planetary dynamics is made as legible as that of the water sediments is now by critical research, chiefly of a mathematico-physical type, aided by laboratory verification. For example, the relations of the earth and moon and their rotations undoubtedly record a history of tidal reactions and of

mutual separation and the reduction of rotations but just how much this may signify in the history of the past and of the future requires a new and much broader inquiry than it has yet received, checked by independent lines of evidence. The question whether the earth is now bodily deformed by tidal stress requires experimental demonstration or overthrow, as an essential factor in this problem This would test the effective rigidity of the earth, and give aid to the solution of other problems. The motion of the moon and the precession of the equinoxes perhaps give lines of approach to the distribution of mass in the earth, and hence to its internal density, etc. The origin of the size, form, constitution, motions and relations of the earth are locked up with the origin of the solar system, and, in general, the deeper terrestrial questions lead out in the end into the realm of cosmology, where the studies of the geologist, astronomer, physicist and chemist blend. Geophysical study must here borrow much from astronomy, but it should make an equivalent return, for the phenomena of the earth are most important factors in cosmology. Any great laboratory of geophysics should do its part in this vast field.

(*d*) *Some of the more specific problems now pressing for solution.*

1. Experiments to demonstrate the diathermacy of the atmosphere and its dependence upon its several constituents, their relations to each other, their ionization, their nucleation, and their other states.

2. Determinations of the gases held in magmas, rocks and meteorites, and the states in which they are held, together with inquiry into the powers of selection and absorption of gases by rocks under ordinary and unusual conditions.

3. Determinations of the functions of the ocean as a reservoir of atmospheric material, involving a study of the relations of its saline constituents to the absorption and release of atmospheric constituents, the relations of temperature and pressure to such absorption and release, as also the functions of vegetable and animal life in the process.

4. Experiments to determine the physical chemistry of natural solutions and precipitates; one important purpose being to furnish a basis for a more comprehensive science of ore deposits.

5. The artificial alteration and recrystallization of minerals under different chemical and physical conditions, in imitation and elucidation of the natural alteration of minerals.

6. The determination of the heat of formation of all natural compounds.

7. Experiments in the deformation of rocks under conditions of great stress, not only in one direction, but with unequal stresses in different directions, and under wide ranges of temperature, moisture and other conditions.

8. Determinations of the relations of pressure to the melting point while under differential stress and other variable conditions, including variable amounts of water, vapors and gases.

9. Determinations of the conductivity of rocks and the laws of variation of such conductivity under varying conditions of heat and pressure.

10. Determinations of the elasticity of rocks and the laws of variation of elasticity under varying conditions of heat, pressure, change of state and change of substance, involving also experiments on the compressibility of rocks.

11. Experiments and mathematical investigations to determine the nature and quantitative value of the possible sources of internal heat under multiple hypotheses as to the original states of the earth.

12. Determinations of the original distributions of heat under such hypotheses, of the secular loss, of the secular generation of heat by gravitative condensation, of the redistribution of internal heat and its possible relations to deformation and vulcanism.

13. Tidal deformation by observational determinations in laboratory and field.

14. Mathematico-physical reinvestigations of the moon earth tidal relationship, and its bearings on the past and prospective history of the earth, wrought out under multiple hypotheses covering the full limits of the probabilities of the case.

15. Tests of the distribution of the internal densities, or mass-distribution of the earth by astronomic data.

16. Gravimetric measures at specially selected significant points embracing (1) such points as will best determine the distribution of gravity upon the ocean areas as distinguished from the continental, and on the border ground between these, and (2) at such points as show notable variations of increase of internal temperature in depth (independent of obvious recent volcanic action) to determine whether the observable variations are dependent on variations of density, and so possibly are dependent on compression.

II. Laboratory of Geophysics in Washington.

In view of the special nature of the geophysical and geochemical experimentation which the elucidation of the profounder problems of the earth requires, and of the absence of many of the requisite appliances in the laboratories now established, we recommend that a central laboratary of geophysics be established by the Carnegie Institution at Washington, and that its scope be broad enough to embrace geochemistry and any other science essentially involved in the problems of the earth. We designate this a central laboratory because we recognize the need of special branch laboratories in various parts of the world for the determination of certain questions which require special localization. We further recognize that alliance and co-operation should be sought with all independent laboratories engaged in any branch of geophysical studies without reference to country. So far as practicable such laboratories should be utilized rather than new branch laboratories be constructed. The central and branch laboratories should be constructed for the special investigations for which they are designed, and should be manned with reference to the problems to be investigated.

If this project be carried out the geophysical work of the world may be harmonized and unified. It is believed that as a certain result geology will soon be placed on a broader and deeper basis, and that an epoch in its history will have been inaugurated of even greater import than any of the past.

For the geophysical work outside of the central laboratory, as already remarked, existing laboratories and the services of men engaged in them should be utilized as far as possible. For instance, in the seismological work—including all earth tremors—a scheme of co-operation should be planned with Milne, Darwin, and many others. In securing the co-operation of independent laboratories now existing, it will doubtless be necessary to subsidize to some extent such as are doing very meritorious work. Often a man engaged in a piece of research is the best qualified to do that particular work. Such a man should be encouraged to do the service which he can most advantageously perform; thus would be utilized the best talent wherever located or however associated. Indeed, it is felt that a vital part of this proposal for a system of Carnegie laboratories of geophysics is the development of a staff and a directory at Washington, which may serve as a center of correlation, through

whose good offices the co-ordination and co-operation of all laboratories doing chemical or physical work bearing upon geology throughout the world may be secured.

(*a*) *Construction of the laboratory.* It seems to us that it would be advisable that the laboratory building should be planned so as to consist of a series of units, not necessarily identical, that can be indefinitely attached to each other with good architectural effects, so that a working portion of the laboratories can be brought into use as early as possible with the minimum expense, while extensions can be made from time to time as funds permit. The units should be so planned that they will be essentially workable in themselves, while at the same time they should be as modest in expense as practicable, so as to give as great adaptability to financial resources as may be, and at the same time permit advantage to be taken of all new developments in buildings and appliances.

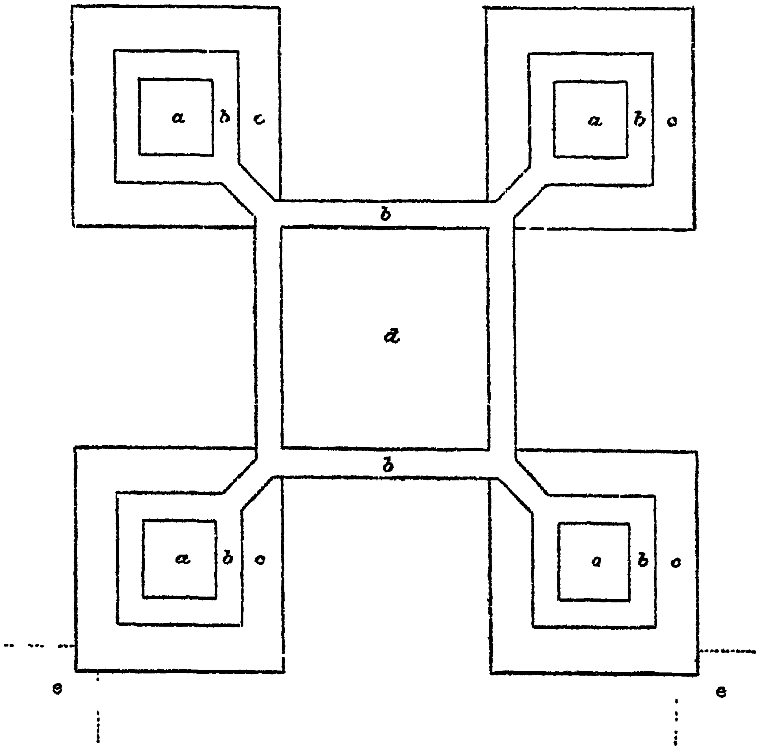
They should provide for uniform-temperature laboratories, rock-bottomed laboratories, ordinary laboratories, workrooms, libraries, offices, etc.

The units should be so intimately united as to be in effect portions of one building, and yet so readily isolable as to exclude undesirable but inevitable effects of work in one class from interference with work in neighboring units.

The exterior walls should be protected against the sun and rain by wide cornices and balconies.

Uniform temperature for experiments, and reasonably uniform temperature for comfortable and effective working should be secured by special means of air control and air supply ; perhaps either by carrying the air (a) through underground conduits, or (b) through special chambers cooled or heated, as required, artificially. The necessity for uniform-temperature laboratories for certain classes of experimentation is recognized, but perhaps not equally the desirability of air controlled in purity, temperature, and moisture for the purpose of securing the highest efficiency of intellectual procedure. The most serious objection to the erection of the laboratories in Washington is climatic. This can probably be overcome in some large measure by artificial appliances, and the importance of securing this as a factor in realizing the highest intellectual results should, in our judgment, receive the earnest consideration of the Trustees.

We submit herewith sketches of two out of the many forms of combination that may be assumed by the proposed unit system of building.



EXPLANATION.

SKETCH I.

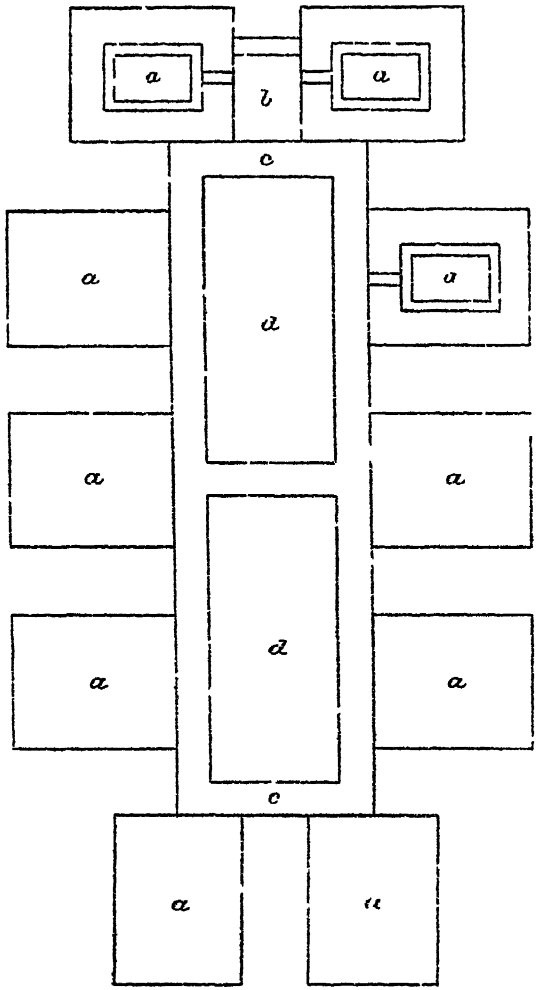
aaaa Uniform temperature laboratories 28′ x 28′, protected by corridors (8 ft., including walls) and work rooms, common laboratories, offices, libraries, etc., *c c c c* outside. To be lighted (a) from above and (b) artificially.

bbbb Corridors for communication, but also to serve as insulators for uniform-temperature laboratories (8 ft., including walls).

cccc Space lighted from without for work rooms, ordinary laboratories, offices, libraries, etc., (18 ft. wide, including outer wall). To be divided by removable partitions (tile or Michaelite) adjusted to needs.

d Central unit that can be treated as the bastion unit, or can be developed as a large (60 ft. x 80 ft.) measurably uniform-temperature laboratory, either lighted from (a) above, (b) laterally through two sets of windows, or (c) artificially.

ee Extension by the addition of units similar to units *a–c*.



EXPLANATION.

SKETCH II.

aaaa Units 80′ x 100′, to be developed as in Sketch I, or variations from that.

b Vestibule hall, stairway, public office, etc. 60′ x 40′.

cc Portico connecting the laboratories on ground and second floors.

dd Courts enclosed by portico. To be an uninterrupted colonnade on inner side facing court, and to be a double colonnade between the buildings, to permit air circulation.

(*b*) *Administration.* The administration of such investigations as are here contemplated should be as simple as possible, consistent with efficiency in the prosecution of research. Those engaged in research work should be as free as may be from the requirements of fiscal business. The success of the enterprise will largely depend upon the men who undertake research, and it may not be easy at first to find men in all respects specially qualified for the work. To some extent they must be developed by the institution itself.

The special methods of administration likewise must be to a large extent developed by the novel scope of the undertaking. Since much must, therefore, be left to future developments, it seems best that the original form of administration should be such as will bring together a large measure of experience and proven competency, and at the same time be such as will permit a change of form with the greatest ease. It is obvious that it will be less difficult to pass from administration by a body to administration by a head than the opposite. It is clear that no one at the outset can have the learning and the breadth necessary to decide on the respective merits of all the problems that will invite investigation, and such a genius may soon arise. For these reasons it would seem best, in the early history of the enterprise at any rate, to vest the control of affairs in a directory or committee made up of scientists who have shown at once administrative abilities, fertility in the development of problems, ingenuity in devising modes of solution and success in the achievement of results.

The directory should meet at least quarterly, and as much oftener as the nature of its duties may require.

The immediate administration of the laboratory should rest in a head or acting head The head should be either a geologist, with a broad and accurate knowledge of many branches of geology, and also well grounded in the principles of physics and chemistry, or else a general physicist, who has deeply studied the broad problems of the earth, and, therefore, has a working knowledge of the principles of geology. The head should be elected by the Trustees, or executive committee upon recommendation of the directory. The election of the staff other than the head may be committed to the directory, or the directory may recommend the staff to be appointed as seems best.

It should be the function of the directory, subject to such rules and limitations as may be determined upon, to consider all proposals for investigation, to devise or mature all general plans for research,

to pass upon all plans for buildings and other appointments needed for research, to recommend the apportionment of funds to research, to buildings, to equipment and to other purposes, to supervise the publication of results, and in general to represent the executive committee in the expert administration of the investigations and of the provisions therefor. They should make such reports to the executive committee as the committee may direct.

It is important that the relations between the executive committee and the directory should be as simple as practicable, and that responsibility should be definitely located, and the proper powers for meeting responsibility should be granted. It is, therefore, suggested that the directory be made responsible for the sub-allotment of such funds as may be appropriated to the several researches. These will inevitably sometimes exceed and sometimes fall short of the estimates made, and in order to secure the best results, the plans adopted will often require modification in the course of their execution. It is obvious that provision should be made for as large a use of scientific discretion in the carrying out of the project as is practicable: especially is this true at the outset, when plans should remain in large measure plastic. In the determination of investigations to be undertaken, and especially in the construction and the manning of the laboratories a slow and conservative course should be followed. Buildings should only be constructed after an exhaustive study of existing laboratories, and a careful consideration of the most promising lines of research. At the outset only those men should be given places in the laboratory who by their services or by their qualities have shown themselves eminently capable of fruitfully carrying on the investigations desired. The relations between the investigations to be taken up in the laboratory and the investigations to be taken up in other laboratories or in the field must be carefully considered individually and collectively, and be so arranged as to be mutually as helpful as possible.

It is inadvisable to map out beforehand a hard and fast apportionment of funds to buildings, equipment and researches. Such an apportionment should be reached step by step as the enterprise develops and the plans approve themselves in actual results. It would seem therefore best that general allotments based upon estimates by the directory should be made for the researches and that the sub-apportionment of these should be left to the directory. For example, if a round sum were set apart to cover buildings, equip-

ment, and researches, the directory could adjust the actual expenditures to buildings, equipment and research in such a way as to secure the best adaptations and most fruitful results. This suggestion would at once simplify the work of the executive committee, while it will confer responsibility and corresponding facilities upon the directory. Under this plan it would be possible for the trustees to fix upon a definite yearly apportionment to the researches indicated.

These suggestions are in the main equally applicable to the administration of researches in geology, physics, and other sciences in respect to which a common plan will doubtless be adopted. They also lead to the consideration of the administrative limits of departments and the working relations between the departments, as these must be determined in selecting formal modes of control

Relative to the definition of departments, their working relations to each other, and the specific forms of administration of the investigations in geology, geophysics, and physics, three plans have been under consideration by the joint committee:

(1) The first plan proposes that there be directories corresponding to the existing advisory committees.

(2) The second plan contemplates the merging of the administration of the investigations in geophysics and geology in a common enterprise, to be administered as a unit by a single directory.

(3) The third plan approved by the committee lies between these two, and proposes a directory for geology and geophysics in two sections of largely identical membership, the section of the directory in charge of investigations in geology to consist of three members, who shall be experienced geologists of wide familiarity with geological problems; the section of the directory in charge of the geophysical laboratory to consist of the above, the head of the proposed laboratory of geophysics as an ex-officio member; also a member of the board of trustees of the Carnegie Institution as an ex-officio member, together with an expert physicist, and an expert chemist, these two to be presumably, though not necessarily, members of the directory in pure physics and pure chemistry, for the purposes of correlation, as above indicated.

This plan contemplates separate allotments for the geophysical and the geological phases of the work to be administered by the two sections of the directory, respectively, but in the closest co-operation, as implied by the large factor of common membership

(*c*) *Expense involved.* In advance of an exhaustive investigation it is impossible to give exact estimates for particular parts of the proposed work; but we are able to give an approximate estimate of the minimum amount which will adequately provide for the entire plan. Our estimates are based on the large idea that a great unoccupied field is to be provided for; but at the same time, on the certainty that with the largest sum which we could reasonably hope would be appropriated, it will be necessary to administer the fund with great care and economy, limiting the investigation to pressing questions of fruitful promise.

At the outset a considerable sum will be necessary for construction, but only a comparatively small sum can be used very wisely for investigation. As the central laboratory approaches completion, the building expenses will be greatly decreased, but the compensation for investigators and the maintenance of the laboratories will necessarily be greatly increased. By proper administration the projects here proposed may be carried out by a uniform annual expenditure.

On the supposition that a system of building by units, as elsewhere stated, is adopted, it is estimated that each unit would cost from $100,000 to $125,000, and its initial equipment from $12,000 to $15,000. It is thought that three units should be erected during the first five years, and two additional during the next five years.

It is estimated that salaries and maintenance, including current cost of experimentation, books, printing, heating, lighting, etc., should rise from $25,000 or $35,000 the first year to the maximum available, which ought to reach $150,000 within the first ten years, to be commensurate with the needs of the field.

An appropriation of $150,000 per year would develop and carry forward the work in a very advantageous manner.

The development of the plan could be somewhat slower than contemplated in these estimates by adding units to the building at greater intervals, which would carry with it a slower increment in the staff and in the cost of maintenance, with, of course, a slower realization of results.

The expense of special laboratories can only be determined as the occasion for them shall be developed. So far as existing laboratories and stations can be utilized, modest auxiliary allotments will usually suffice.

Estimate of cost of laboratories and running expenses per annum, $150,000.

We submit herewith two appendices, viz :—

Appendix 1. A carefully detailed estimate for the proposed central laboratory, prepared by Dr. George F. Becker, which does not, however, cover all lines of research contemplated in our report.

Appendix 2. Communications from distinguished scientists of different fields relative to the value and functions of a geophysical laboratory.

In conclusion, we beg to express to the trustees of the Carnegie Institution our profound appreciation of the greatness of the enterprise they have in charge. We believe it not too much to think that it will mark a new era in the intellectual development of the race, an era in which scientific research will hold the foremost place in the agencies of progress and, we may hope, will be given the highest place in the esteem of mankind.

We venture to believe, also, that if the facilities herein recommended are granted, it will assure to America the foremost place in the special fields for which, through your kindness, we have been permitted to plan.

Respectfully submitted,

R. S. WOODWARD, *Chairman*,
CARL BARUS,
T. C. CHAMBERLIN,
A. A. MICHELSON,
C. R. VAN HISE,
CHAS. D. WALCOTT.
Committee.

SEPTEMBER 23, 1902.

APPENDIX 1 TO REPORT OF ADVISORY COMMITTEE ON GEOPHYSICS

PROJECT FOR A GEOPHYSICAL LABORATORY.

By George F Becker.

Contents.

Part I.—Outline of Geophysical Research.

Part II.—Plant Required.

Part I. Outline of Geophysical Research.

1. Scope of the undertaking.—The purpose of a geophysical laboratory is to supply a firm scientific foundation for the study of the past history and the present condition of the earth. The researches in such a laboratory can consist only in part in the application of known theories to terrestrial problems, because in many most important cases the theoretical basis is imperfect or altogether wanting.

Geophysics covers a distinct group of problems which are so closely related to one another in their nature and in the methods applicable to their solution as to stand somewhat apart from general physics. These problems demand for their successful solution simultaneous study and the carefully organized co-operation of a number of investigators over a series of years.

It is because these conditions have hitherto been lacking that so much remains to be done in this direction.

2. *Some of the problems of geophysics.*—To illustrate this view of the matter brief comments may be submitted on three of the many great questions of geophysics, namely, the distribution of terrestrial density, upheaval and subsidence, and vulcanism. All of them are subjects of extreme difficulty, but not, in my opinion, beyond the reach of well directed efforts.

The distribution of densities evidently depends upon the materials of which the globe is composed, the temperatures which prevail at different depths, and the elastic properties of matter The possible laws of terrestrial density cannot be intelligently discussed beyond the point where Laplace left the subject until the law of elasticity for finite stresses has been elucidated.

Some suggestive work on elasticity has recently been done in the Division of Chemical and Physical Research of the U. S. Geological Survey along the lines of a former investigation of my own, and is still in progress there. While a large amount of confirmatory experimentation is still requisite, enough has been accomplished to indicate the character of the result.

The law of elasticity for constant temperatures having been discovered, it will then be necessary to determine the functional relations of elasticity and temperature. And since the range of temperature is necessarily large, considerable researches at high temperatures must accompany the study of the finite stress-strain function.

Supposing the relations known, a comparison of the earth, the moon and Mars can be employed to test the more probable theories of composition ; for example, that which considers the planets, like the meteorites, to be composed of nickel-steel and stony matter comparable with the average rock.

The theory of the vibrations of a sphere, when adequately developed, can be used still further to confirm or to upset working hypotheses as to the chief terrestrial constituents by taking advantage of the rate of transmission of earthquake shocks along various chords of the terrestrial sphere.

Evidently therefore the problem of the distribution of densities requires the combined efforts of at least three investigators—one devoted to elasticity, a second to high temperatures, and a third to the mathematical development.

The most hopeful line of attack on the problem of upheaval and subsidence appears to me to be as follows : A sphere is conceivable in which the distribution of thermal diffusivities and conductivi-

ties is such that the condition called by Fourier "the steady flow of heat," would subsist. In such a globe cooling would be accompanied by no superficial deformation. It is therefore essential to the solution of the problem that the character of such an ideal, undeformed sphere should be worked out from the mathematical side. If it can be shown that the constitution of the earth does not conincide with that of this hypothetical globe, a reason for deformation will have been discovered. As soon as an approximate solution has been attained for the constituent materials of the globe and their properties, then comparison with the hypothetical sphere of no deformation will show the general character of the deformation which should be expected in the earth.

It appears possible that some of the greater phenomena which must be discussed under the head of upheaval and subsidence are not connected with the cooling of the globe, but with the retardation of its rotation and the consequent changes in its form in maintaining equilibrium. It is a very remarkable fact, not recorded in literature, so far as I know, that most of the great continental outlines lie approximately on great circles which are nearly tangent to the Antarctic Continent and the Arctic Ocean. It seems possible that these great coast lines answer to the directions of rupture of a spheroid retarded by tidal action. A proper discussion of the matter requires a knowledge of the constituents of the earth and of the laws of rupture, which are as yet in a most unsatisfactory condition.

Orogenic dynamics is a mere branch of upheaval and subsidence involving the theory of elasticity, plasticity, and rupture for finite strains. It must explain the origin of joints, systems of veins, slaty or schistose cleavage and the simpler flexures.

It is evident, then, that the problem of upheaval and subsidence is linked by the closest ties to the problem of the earth's constitution and should be conducted by the same corps of physicists.

Our knowledge of the operative causes in vulcanism is of the vaguest description. It seems almost certain, however, that these impressive thermal phenomena cannot be wholly independent of the vast amount of energy dissipated in the orogenic upheavals which so often accompany eruptivity. The thermodynamic side of this problem therefore demands earnest study.

Vulcanism also raises other and very difficult questions. That magmas are solutions is known, but scarcely anything else is known about them. Thus, vulcanism implies researches on the

nature of igneous solutions, their chemical affinities, their ionization, latent heat, eutectic properties, etc. A special branch of this subject, but one of the most important, is the study of aquo-igneous fusion and the solutions resulting from it. The study of eutectic mixtures, including those into which the hydroxyl enters as a component, should lead to a classification of massive rocks on a new basis. Vulcanism also demands a study of the viscosity and diffusivity of solutions, especially of magmas, and of the relations which must subsist between them. The results obtained should be determinative with reference to the theory of the so-called "differentiation" of rock magmas. Diffusivity is inversely proportional to some function of viscosity, and there is some probability that this function is the square. Preparations are now in progress to test these relations in my laboratory by novel methods.

The cause of the extrusion or the intrusion of magmas is unknown. I am of the opinion at present that its origin is the elastic pressure of solid masses upon materials deprived of their rigidity by fusion. This may at least serve as a starting point for investigation. It appears then that vulcanism is probably allied to orogeny in an intimate manner.

Its study will require at least one additional physicist devoted to researches on the physical properties of solutions and of a physical chemist to undertake the more purely chemical properties of magmas.

3. Necessity for Organized Research in Geophysics.—This brief outline of the possibilities of systematic investigation in three of the most important problems of geophysics appears to me to justify the opinion that a special corps of investigators, with special laboratory facilities and under a single directorship, should be devoted to the study of geophysics. In my opinion, it would be beyond the power of any one man to co-ordinate the various branches of the work of a geophysical laboratory, while paying due regard to the needs of geological science, and at the same time to superintend any more extensive scheme of physical research. To be successful the work must be organized with reference to its special character, and even then the task will severely tax the best corps of men which can be enlisted. On the other hand, it is difficult to imagine contributions to general physics of more fundamental importance than those which would ensue from the successful prosecution of these geophysical researches.

4. Personnel Required.—Assuming, then, that a special laboratory, staff and director are requisite to the successful prosecution of geophysical researches, I offer the following estimates of the personnel and plant appropriate to such an institution, basing the plans largely upon the experience of the Physikalische Reichsanstalt of Charlottenburg, with modifications adapting them to American conditions.

The plan is in general terms as follows: In addition to a director, one mathematician, four experimental physical investigators, one chemical investigator and one analytical chemist are recommended. The necessity for a first-class mathematician is too evident to need comment. For years to come one physicist should confine himself to the study of elasticity, plasticity and rupture. A second physicist is required to take charge of high temperature work, beginning with an extension of thermometry to the melting point of platinum and the quantitative determination of the fundamental physical relations at those temperatures.

A third physicist should devote himself to the study of viscosity and diffusion, beginning with solutions at ordinary temperatures. Osmosis, with inorganic septa, and capillarity, two topics of very great importance to geophysics, should be entrusted to a fourth investigator. A complete geophysical laboratory must include at least one chemist to study the chemical relations of eutetic mixtures and investigate affinities at high temperatures. An analytical chemist is required for the numerous chemical analyses which will be called for in all branches of the work.

This corps of investigators must be provided with assistants to relieve them of the simpler details. Mechanical assistants and a small office force will also be required.

In estimating for the salaries of the principal members of the staff, I have taken as a basis the best salaries paid to college professors. Unless such salaries are paid there would be danger that the men might be tempted to abandon geophysics for college positions, much to the detriment of the proposed research. Investigators in geophysics must learn to take a somewhat novel attitude towards the science of the earth. They must attain a sufficient grasp of geology and its phenomena to perceive the demand for physical research, the application of its results and their relative importance. A man who has attained this unusual standpoint cannot readily be replaced either by a physicist unacquainted with geology or by a geologist insufficiently trained in physics.

I do not think that a man competent to investigate great problems should be asked to serve for less than $3,000 a year. Men who have earned large reputations must receive higher salaries, rising, say, to $6,000. The director might be worth $7,000. It is my understanding that some system of retiring pay is to be provided in the Carnegie Institution. I would suggest that if a fixed age of retirement is adopted, provision be made for exceptions in cases of unusual vigor, and, further, that retired officers should be expected to contribute to the publications of the Institution such material as they may find practicable without undue exertion.

5. *General features of plant.*—A geophysical laboratory to be satisfactory must be built upon a rock foundation in a locality as far removed as practicable from all mechanical and electrical disturbances (1,000 feet at least), and must be secure against the encroachment of disturbing conditions for the future. The success of secular experiments, the stability of instruments and the accuracy of electrical methods of measurement demand such conditions.

A satisfactory laboratory cannot be built in a brief period, and I have therefore given estimates for the distribution of the expenditures for plant, salaries and maintenance over a term of four years, supposing the fourth year to represent the permanent annual expenditure. These estimates would, of course, require careful revision by an architect and others, but may serve a preliminary purpose.

In closing what I can at present contribute to the subject, I have outlined some features of such an administration as experience seems to show should be adopted for the geophysical laboratory.

6. *Scientific staff.*

YEAR.	PERMANENT STAFF.		TEMPORARY STAFF.	
First	Director	$6,000–8,000	Computor	$1,000–1,500
	Mathematician	3,000–6,000		
	Physicist	5,000–6,000		
	Chemist	3,000–6,000		
Second	Director	6,000–8,000	Computor	1,000–1,500
	Mathematician	3,000–6,000	Ass't Phys.	1,000–1,800
	Physicist	3,000–6,000	Ass't Chem.	1,000–1,800
	Chemist	3,000–6,000		

Scientific staff—Continued.

YEAR.	PERMANENT STAFF		TEMPORARY STAFF	
Third..	Director..	6,000–8,000	Computor	1,000–1,500
	Mathematician	3,000–6,000	Ass't Phys.	1,000–1,800
	Physicist	3,000–6,000	Ass't Phys.	1,000–1,800
	Physicist	3,000–6,000	Ass't Phys.	1,000–1,800
	Physicist	3,000–6,000	Ass't Chem.	1,000–1,800
	Chemist	3,000–6,000	Ass't Chem.	1,000–1,500
	Analyt. Chemist	2,000–3,000		
Fourth and following years.	Director	6,000–8,000	Computor	1,000–1,500
	Mathematician	3,000–6,000	Ass't Phys.	1,000–1,800
	Physicist	3,000–6,000	Ass't Phys.	1,000–1,800
	Physicist	3,000–6,000	Ass't Phys.	1,000–1,800
	Physicist	3,000–6,000	Ass't Phys.	1,000–1,800
	Physicist	3,000–6,000	Ass't Chem.	1,000–1,800
	Chemist	3,000–6,000	Ass't Chem.	1,000–1,500
	Analyt. Chemist	2,000–3,000		

Total salaries of scientific staff.

First year	$15,000–26,000	$1,000–1,500	$16,000–27,500
Second year	15,000–26,000	3,000–5,100	18,000–31,100
Third year	23,000–41,000	6,000–10,200	29,000–51,200
Fourth year	26,000–47,000	7,000–12,000	33,000–59,000
Permanent pay roll	....	....	33,000–59,000

The reasons for the segregation by years are as follows:

The mathematician, requiring no apparatus beyond an arithmometer, cannot begin his labors too soon.

The services of the director, a physicist and a chemist will be needed in planning the laboratory and initiating steps for its equipment. These men will therefore be needed from the commencement of the enterprise. During the second year some facilities should be available for research work for the physicist and the chemist, though only certain lines of inquiry would be open to them. It is estimated that at the end of two years the building would be sufficiently completed to permit of the employment of nearly the whole staff, partly in installing the final equipment and partly in research.

In estimating for the mechanical and clerical force required I have also supposed that employes will be engaged only when needed, as shown in the following table:

7. Clerical staff and mechanics.

YEAR.	MECHANICAL FORCE	CLERICAL FORCE.
First.....	Expert Mechanic......$1,500–1,800	Stenographer....$600–1,200 Stenographer.... 600–1,200
Second..	Expert Mechanic 1,500–1 800	Stenographer.... 600–1,200 Stenographer.... 600–1,200
Third.....	Expert Mechanic......1,500–1,800 Instrument Maker.... 600–1,000 Instrument Maker. .. 600–1,000 Electrician.... ...... 600–1,000 Messenger... .... 480– 600 Charwoman.......... 240 Charwoman......... . 240	Stenographer... 600–1,200 Stenographer... 600–1,200 Stenographer ... 600–1,200
Fourth ...	Expert Mechanic......1,500–1,800 Instrument Maker..... 600–1,000 Instrument Maker..... 600–1,000 Electrician..... . .... 600–1,000 Mechanic........... . 600–1,000 Messenger........... 480– 600 Messenger........... 480– 600 Charwoman.......... 240 Charwoman.... 240	Stenographer.. . 600–1,200 Stenographer.... 600–1,200 Stenographer.... 600–1,200

Pay-roll of clerical staff and mechanics.

First Year.................. ..	$1,500–1,800	$1,200–2,400	$2,700– 4 200
Second Year.................	1,500–1,800	1,200–2,400	2,700– 4,200
Third Year................	3,780–5,880	1,800–3,600	5,580– 9,480
Fourth and following......... ..	4,800–7,480	1,800–3,600	6,660–11,089
Permanent Pay-roll......	....	....	6,660–11,080

8. Estimate for Plant.

First Year.	Toward construction of main building...............	$100,000
Second Year.	Toward construction of main building....	100,000
Third Year.	Completion of main building.........................	50,000
	Equipment..	40,000
	Library..	6,000
	Total (distributed over three years)............................	$296,000

9. Maintenance—Years.

	First.	Second.	Third.	Fourth.
Experimental research..............$6,000		$10,000	$15,000	$20,000
Library, printing and sundries.......	2,500	2,500	4,000	5,000
Total by years.................	$8,500	$12,500	$19,000	$25,000

10. Summary of estimates.

	Salaries.	Plant	Maintenance.	Totals (assuming mean salaries).
First year..	$18,700—31,700	100,000	8,500	$133,700
Second year.................	20,700—35,300	100,000	12,500	140,500
Third year...	34,580—60,680	96,000	19,000	162,630
Fourth and following years....	39,660—70,080	....	25,000	79,870

These estimates assume that land, power, heat, administrative expenses and maintenance of buildings are provided for in the general organization of the Institution.

11. Organization.—I take the liberty of recording here some notes on such an organization as appears to me suitable for a geophysical laboratory, established upon the Carnegie Foundation. Such a laboratory would naturally be under the control of the Executive Committee, but I would suggest that its immediate governing board consist of an independent visiting committee, comprising three physicists and three geologists not connected with the Institution, this board to be presided over by the director of the laboratory, who shall vote only in case of a tie.

The members of the visiting committee might be appointed for four-year terms, retiring in rotation. They might meet annually at a stated season, to consider the report of the director, and also, in urgent cases, upon call with due notice. Business requiring the attention of the committee between annual meetings, if not very urgent or complex, might be settled by correspondence. The members of the committee might receive their expenses and a per diem compensation for attendance at the meetings.

The director might lay before the committee at its annual meetings:

(1) A report of the scientific work of the year.

(2) A plan for scientific work for the ensuing year.

(3) Plans for the expenditure of funds for the ensuing year; such plans being subject to the approval of the committee, excepting that one-fifth of the sum appropriated for research should constitute a contingent fund to be expended at the discretion of the director and duly accounted for by him.

(4) Nominations for vacancies in the permanent staff.

(5) Recommendations for the reception of volunteer assistants or visitors, whose co-operation may be desirable.

The decision of the visiting committee in matters pertaining to the salary of permanent members of the scientific staff should be final.

The director might further submit an annual report to the Executive Committee of all expenditures proposed for the approaching year, after this has received the approval of the visiting committee, and a report of the funds expended during the year upon the plan previously approved both by the visiting committee and the executive committee.

All appointments, excepting in the permanent scientific staff, might be considered temporary and terminable at the discretion of the director.

All expenditures might be subject to the director's order, he being responsible to the executive committee of the institute for all outlays.

All powers not directly exercised by the executive committee or the visiting committee would be vested in the director.

Part II.—Plant required.

12. *Situation desired.*—In the foregoing an effort has been made to show that these fundamental researches in geophysics are of a character to demand the combined efforts of several investigators, and a special laboratory suitably located, planned, and equipped for this work.

Some of the requirements of such a laboratory may be anticipated at once from the character of the problems to be considered, others are suggested by the German Reichsanstalt, where for several years research work of a high order has been carried on in pure physics ; the final details can only be prepared with the help of an architect and after visiting the modern research laboratories.

The essential conditions for the prosecution of the work here contemplated are :

(1) The greatest possible freedom from mechanical and electrical disturbances.

(2) Effective provision against changes of temperature.

The first necessity is, therefore, the choice of a suitable location. The site selected should be high, to insure good light and dryness. and should offer a rock foundation for the laboratory building. It

should be remote from powerful electrical or mechanical plants, and should be surrounded by sufficient land under its control to secure the institution against the encroachment of such sources of disturbance for the future.

It is hardly possible that the present rapid extension of electrical tramways to place such a laboratory permanently beyond the reach of all electrical influence without removing to a point so inaccessible as seriously to inconvenience the daily life of those engaged in the work. Fortunately, however, the number of problems requiring conditions of extreme freedom from electrical disturbance is rather limited, and these have been left by common consent to small laboratories far removed from the centers of population, and equipped solely for this work. The extreme requirement in this direction may, therefore, be regarded as superfluous in the present plan.

The laboratory may be safely located within one thousand feet of a well insulated, underground trolly, or a *double* overhead system; ordinary overhead feeds, with return through surface conductors or through the rails, will cause a considerable magnetic disturbance at a distance of a mile with the passage of every car. Cars equipped with accumulators, electrical carriages, or systems of electrical lighting using direct current, exert little influence at a distance.

Nearly all the finer physical measurements are seriously disturbed by the jar of passing traffic, and by temperature changes. It is, therefore, most important that the laboratory building be reasonably remote from paved thoroughfares, from foundries and plants where heavy manufacturing is done, and that it be so constructed as to protect it, as far as may be, from temperature variations

This applies as well with respect to the sources of electrical and mechanical power for the laboratory itself, and to its machine shop, dynamos, air compressers, refrigerating machinery, to rooms for carrying on such special researches, themselves as require powerful machinery, steam pressure, gas furnaces, or anything by which the observatory building or other work in progress there, could be disturbed. These would, therefore, be best located in the general power house of the institution at some distance (200 feet or more) from the main laboratory.

There are two methods of attacking the problem of constant temperature, both of which are necessary to a successful result. The one involves the circulation of artificially cooled or heated air with the help of suitable regulating devices, the other that the building be constructed in such a way that outside temperature changes pro-

duce a minimum effect within. The first is accomplished by proper heaters, refrigerating machinery and automatic regulators. The second is a factor which must enter into the construction of the building from the outset and will add materially to its cost.

13. General character of laboratory building. — It is here that the experience of the Reichsanstalt is most valuable. The arrangement consists in a general way of central isolated rooms, one on each floor, with double doors, double walls and double glass floors above and below, thus giving inclosed air spaces on all sides of each room. Around this isolated room on each floor is a corridor. Neither the central rooms nor the corridor are connected with the heating plant of the building. Opening outward from this corridor is a series of rooms for general work, extending completely around the building. These are heated and ventilated from the plant mentioned above, which should be able to maintain the temperature constant within four or five centrigrade degrees throughout the year. Beyond these rooms is the outside wall of the building, of considerable thickness, and rendered insulating by means of perforated or porous brick, mineral wool or other suitable material.

It is plain that this construction must furnish the most perfect control of the temperature conditions which is possible in a building where many men are at work :—an outside insulating wall, the general laboratory rooms where the temperature is maintained constant by the circulation of air of constant temperature, a corridor which is really exposed to no temperature change, and finally, again inclosed within double insulating walls, an innermost room where the most refined experiments can be conducted without danger of temperature disturbance other than that from the body of the observer against which special provision is necessary to fit the conditions which obtain in each case.

Protection from the heat of the sun on the top of the building is secured by following the same general plan. So much of the roof as covers rooms lighted by side windows, i. e., the general laboratory rooms above described, would require to be double, and contain a thick layer of insulating material. The central rooms are dependent on overhead light, and the roof immediately over them must therefore be of glass, also double, and protected from the direct sunlight by a metal or tile roof, raised three or four feet above the glass to admit the light with the minimum of heat, somewhat as indicated in the accompanying sketch.*

* Here omitted.

A sub-basement, in which air circulates at a fairly uniform temperature, is also essential to dryness and constant temperature in the main basement where the larger permanent apparatus, for which stability is essential, is mounted. Within this sub-basement, inclosed by properly insulating walls, one or two underground chambers would be provided for such secular experiments as require constant conditions for longer periods of time, like diffusion experiments in viscous media. Such rooms require to be visited by the observer only at long intervals.

Vertical shafts for experimental purposes and special ventilation could be provided by towers at the corners of the building. Ordinary ventilation, hood flues, etc., would be carried by the outside walls, so far as practicable.

14. Floor space required.—A building of this character, to meet the needs of the work contemplated, would contain three (3) working floors above the subbasement. The first, or main basement, would contain the stone piers and would mount all apparatus requiring great stability. The next, or main floor, would contain the library, director's room, and general laboratory rooms; the upper floor the chemical laboratary, photographic laboratory, and work rooms for such apparatus as does not require extreme stability. At least a part of the roof should be available for mounting special apparatus out of doors.

This building would cover some 12.000 feet of ground over all, which, taking the distribution of space followed in the Reichsanstalt, would give a net floor space, exclusive of halls and corridors, of some 16,000 feet, of which perhaps 4,000 would be taken up with library, store rooms, balance room, clock room, mercury room, toilet rooms, offices, etc., which would be used by the entire staff in common, leaving 12,000 feet to be placed at the disposal of the director and the five investigators, and for reserve space in anticipation of future need. Approximately 1,500 feet would therefore be available for each investigator, with his assistant and mechanical helpers, for all the problems upon which he might be engaged, with 3,000 feet in reserve.

15. Special construction of building.—One other feature of the Reichsanstalt has proved of especial value, viz: The arched construction, which is maintained throughout the building, and enables the partitions between adjoining laboratory rooms to be taken away and two or more rooms thrown into one to meet the requirements of a particular experiment. Each arch section, or smallest

contemplated work room, contains complete heat, gas, water and electrical connections, and is thus potentially independent.

16. Necessity for special power house.—The needs of the laboratory in the matter of electric and other power will require careful consideration before the final plans are prepared. Electricity for charging storage batteries may be taken from any direct current supply wires of proper voltage, i. e., preferably 110 or 120 volts. Higher voltages involve special insulation and a great waste of power, unless an unusual number of cells are arranged to be charged simultaneously, which would be most inconvenient in a laboratory of this character. Street car feeds are not suitable for such a purpose; the voltage is high (usually 500 volts) and widely variable with the amount of traffic. Furthermore, if the distance from the power house were large, no considerable supply could be furnished to the laboratory without increasing the size of the feeds—an item of unnecessarily large expense—or interfering with the supply for the cars, a possible source of dissatisfaction, both on the part of the railway company and the laboratory.

Alternating current cannot be stored as such, and is, therefore, best produced by special dynamos under the control of the institution; the variations in a general supply main would be most disturbing. An exception might be made in the case of very high potentials, where the cost of a special wire from the power plant would be small, and the current could then be controlled by suitable transformers in the laboratory.

It will certainly be more satisfactory and probably cheaper for the institution to control its own electrical plant, especially as both alternating and direct current is likely to be needed, and at varying voltages, which would hardly be obtainable from outside.

17. Summary of Part II.—The general requirements may now be summed up as follows:

I. A site, offering—
   (1) A rock foundation.
   (2) Permanent freedom from mechanical and electrical disturbance.
   (3) Sufficient elevation to insure dryness and good light.

II. A two-story building with basement covering approximately 12,000 feet of ground, and offering—
   (1) A stable foundation for apparatus to which extreme stability is essential.
   (2) A thoroughly insulated construction for controlling the temperature conditions within.

III. A power house at some distance (at least 200 feet) from the laboratory, to contain the machine shop, steam power, dynamos, storage batteries, the heating and refrigerating plants, rooms for special researches likely to involve tremors or electrical disturbances.

APPENDIX 2 TO REPORT OF ADVISORY COMMITTEE ON GEOPHYSICS.

LETTERS FROM EUROPEAN SCIENTISTS RELATIVE TO RESEARCH IN GEOPHYSICS.

CONTENTS.

In response to a letter of inquiry from the Secretary of the Committee on Geophysics, a number of European scientists kindly gave their views on a laboratory for geophysical research.

[*Prof. H. Poincaré to Mr. Walcott, May 27, 1902.*]

[Translation.]

SORBORNE, PARIS, FRANCE, *May 27, 1902.*

DEAR SIR:

There is no doubt that advances in the physics of the globe will be of the highest interest, as well from a theoretical as from a practical point of view. I believe that useful work could be done in this direction, and with a prospect of success, if a man competent in such matters would undertake the direction of the work, and were to have at his disposition sufficient resources and a sufficietn number of assistants.

Please accept, my dear sir, the assurance of my distinguished esteem.

H. POINCARÉ.

To C. D. WALCOTT,

Secretary Carnegie Institution, Washington D. C.

[*Lord Kelvin to Mr. Walcott, June 2, 1902.*]

15, EATON PLACE,
LONDON, S. W., *June 2, 1902.*

DEAR MR. WALCOTT:

I was sorry not to have time to answer your letter of April 24, regarding the proposed establishment of a geophysical laboratory, before I left America. I had not forgotten it, and was on the point of writing to you when your letter of May 15, connected with the same subject, reached me here.

I am very glad to hear that there is a prospect of a geophysical laboratory being founded as part of the Carnegie Institution of Washington. I think it may prove most beneficial. Observations of volcanic phenomena in all parts of the world might, I think, be largely promoted by such an institution. Since I had the pleasure of meeting you in Washington we have had a sad and terrible demonstration in Martinique and St. Vincent of the great human interests concerned, which should prove a great impulse to prosecuting the natural history study of the subject.

I suppose you know of Milne's seismographic work, which he commenced in Japan and is now continuing in the Isle of Wight. Such work might very properly be taken in hand by the Carnegie geophysical laboratory. Professor T. Gray, Rose Polytechnic Institute, Terre Haute, Indiana, would, I am sure, be able and pleased to give you good advice on the subject. He worked in conjunction with Milne on it in Japan.

In respect to purely laboratory work the melting temperatures of all kinds of natural rocks is a very important subject for experiment. So is the relative density of melted rock and of solid rock at the same or at slightly lower temperature. This could be done most easily by melting a considerable quantity of rock in a crucible and dropping into it pieces of the same rock solid, but previously heated up to nearly the same temperature. It should not be a difficult observation to find whether they sink or swim; except in cases in which the melted rock is frothy at the surface by emission of gas.

By book post I send you with this an article on The Age of the Earth, which you may have seen already, but probably without the addendum at the end, which bears on geophysical laboratory work. You will also see on page 75 of the print a reference to good work by Dr. Carl Barus in Mr. Clemence King's geophysical laboratory.

We have all been very sorry that circumstances did not allow the continuation and extension of that laboratory, and I am sure it will be a good thing if it can be followed up in the manner you propose. I cannot but think that Mr. Carnegie himself will be interested in the subject.

In answer to your letter of May 15, I am sorry I do not know enough to allow me to recommend anyone as specially well qualified for conducting the work.

Yours sincerely,

KELVIN.

[*E. Suess to Mr. Walcott, June 7, 1902.*]

VIENNA, *June 7, 1902.*

DEAR SIR:

Your very kind letter, dated May 5, has occupied me very much during the last days. There is danger in proposing experiments which must be conducted under conditions too different from those of nature. I hope I have not gone beyond your intentions in inviting two distinguished friends, one our ex-professor of physics, Mr. Mach, and the other our professor of petrography, Professor Becke, to a confidential discussion of your question. Experiments are now being made on melting points at Gratz (Doelter) and at Geneva (Brin), and are in preparation here. You know the brilliant experiments on cooling by Sir Robert Austen. But it seemed to my friends as well as to myself that our knowledge is singularly deficient in regard to the influence of pressure on the melting point, as well as on increase or decrease of solubility. For the sake of attaining exactitude of expression, I asked my friends to give me their opinions in a few written lines, and these I beg to append to this letter.

But I must confess that quite a flood of different pieces of work and questions of the most varied character arise before my mind. I beg permission to speak of one of these tasks.

It is a very curious fact, that any brick or piece of pottery during baking attains, and then retains for all time, the magnetic orientation of the place of baking. Every fragment of brick or of old pottery gives the magnetic meridian of the place and time of baking, not the declination, because the situation during baking is not known. The variation of the magnetic meridian during the last thirty or forty years can easily be read from our bricks, and if the

laws of magnetic variation were more accurately known, we should possess a curious new chronological method. French naturalists have begun to examine baked clays from the lava fields of the Auvergne, and in this case declination, as well as magnetic meridian, can be examined.

These French observations, contained in the Comptes Rendus for 1891 are a small beginning of a great task. This task is rendered somewhat more difficult by the circumstance that in taking samples of baked clays their present geographical position, say beneath a coulee of lava, cannot be accurately fixed by compass, the needle being disturbed by surrounding masses. so that true north in every case must be fixed by simple astronomical methods. It is quite superfluous to say that intense interest would be awakened by the examination of such baked clays from high latitudes, say, from the basaltic region of Disko, or any boreal district. All questions relating to a variability in the position of the poles, etc., are thus raised. So, too, a series of observations from superposed beds of one and the same region, for example the Cascades, would be highly instructive, and a series of observations from the West Indies or Mexico to boreal districts would be highly meritorious work, and, so far as I can see, within the scope of your institution. Perhaps this may in future prove to be the way to attain a positive chronology for certain geological events, but the seemingly periodic character of magnetic variation will. I fear, turn out a difficulty which only the coming generation may overcome.

I remain, dear sir, respectfully, and full of sincere envy,

Yours,

E. SUESS.

[*Dr. Ernst Mach to E. Suess, May 29, 1902.*]

VIENNA, *May 29, 1902.*

MY DEAR MR. PRESIDENT :

I transmit herewith the letter which I was unable to give you yesterday, and also a few lines on the question there touched upon: *How can Geophysics best be promoted?* The question is easier put than answered. It cannot be denied, however, that the determination of the effects which the highest attainable pressures, as well as the highest and lowest attainable temperatures, exert upon the melting point or the freezing point, the latent heat, the specific

heat, the conductivity, viscosity, and other physical constants of the terrestrial components would greatly advance geophysics. Mr. S. Guenther, professor of geography at the Technical High School in Munich, who has done much work in geophysics, might be more familiar with these questions

With expressions of sincerest respect, your most obedient.

DR. ERNST MACH.

[*F. Becke to E. Suess, June 6, 1902.*]

VIENNA, AUSTRIA, *June 6, 1902.*

MY DEAR MR. PRESIDENT:

In accordance with your wish I take the liberty of communicating to you the following project for experiments bearing upon the development of slaty structure in crystalline schists.

According to a law announced by Riecke in Goettingen early in the nineties, the melting point is raised by mechanical deformation, or what is the same thing, the solubility of a body in the surrounding solution is promoted so that if in a saturated solution two bodies of the same kind as that dissolved exist, of which one is subjected to mechanical pressure (or is stretched or twisted), while the other remains in a state of ease, the strained body is dissolved, while the unstrained body grows at the expense of the same solution.

This law may be applied to an aggregate of crystals surrounded by a saturated solution of their substance. If the aggregate is subjected to a one-sided pressure, the portions under pressure should grow. The crystals should flatten perpendicularly to the direction of pressure, and a slaty structure should result.

Thus one might make a cake of salt soluble in water, such as alum, epsom salts, etc., and subject it to a one-sided pressure in such a manner as to permit of lateral yielding, the vessel meantime being filled with a saturated solution of the salt.

It would be particularly interesting to investigate the influence which especially favorable directions of growth might exert; how, for example, on the one hand isometric crystals would behave and, on the other hand, such as have a natural tendency to a tabular or prismatic form.

With the highest respect, your obedient

F. BECKE.

[*Dr. O. Kohlrausch to Mr. Walcott, June 15, 1902.*]

[Translation.]

CHARLOTTENBURG, *June 15, 1902.*

HONORED SIR:

Although your inquiry is directed to the physicist rather than to the geologist, a competent judgment from the physical standpoint will require at least a superficial knowledge of geological questions and of the geological point of view. For this reason my answer has been delayed.

With the help of the literature and from conversation and correspondence with colleagues, I think I am now sufficiently informed upon the question to be able to say without reservation that a physical laboratory for geological purposes may achieve great success. So far as known to me such an institution would be among those laboratories which at present are subject to no competition. This is an important point. For if such amply endowed institutions are to be created for physical research, as are now contemplated in various parts of the world, an effort should be made to distribute the problems among them so far as it is possible. Each nation will, of course, reserve to itself many fields of scientific investigation, as well as the application of the results, whether similar investigations are in progress elsewhere or not; I might name as such, for example, thermal, electrical and optical tests of instruments and materials in addition to the more general scientific investigations. But there is such a strong tendency continually to subdivide the field of scientific research, and each subdivision when undertaken upon a large scale requires so large an expenditure of funds and labor, that economic considerations make it desirable to divide up the task in undertaking the various special fields of research The results of such investigation, of course, become international property and duplication is seldom necessary.

For this reason it seems to me an important argument for the founding of a geophysical laboratory within the Carnegie institution, that there is not now in existence an institute with large resources, dedicated to this purpose.

It is furthermore a conspicuous fact that the United States, by reason of its extended and widely varied natural resources, would offer an especially favorable opportunity for a geophysical institute.

The chief problem of the institution will be to establish the con-

ditions under which the component parts of the earth form and have been formed High temperatures and pressures will therefore be called upon at once. Physics is, no doubt, still far removed from meeting all the needs of geology in this direction. Even though 3,000 atmospheres and 3,000 degrees Celsius represent imposing figures, and these magnitudes not only can be reached, but are already or in the immediate future will be measurable, in the *combination* of pressure and temperature, we must be content at first with more modest dimensions. But we shall certainly be indebted for advances in these problems to just the very stimulus arising from geophysical investigations. Electrical methods of heating and of temperature measurements are of very recent date and are still capable of much further extension. Bolometrical, and especially optical methods of pyrometry also promise important developments.

But even with the present limitations elasticity and rigidity, plasticity melting points and their dependence upon psessure, vapor tension, critical temperature and the chemical relations, especially to water and carbonic acid, offer fields of apparently unlimited scope where as yet little has been done. Mutual solubility and crystallization form solutions, i. e., the separation of magmas into their component parts, are already within the reach of comparatively simple resources, and will certainly lead to most important results.

Electrolysis, which, having been cumulatively in operation for thousands of years, and must have been an effective force in the formation of the earth, still remains an entirely virgin field from a geological standpoint. Thermoelectric and other electrical potentials and the currents developed in the body of the earth by them, can scarcely be said to have been studied with reference to geological questions.

Magnetism, the measure of the earth's attraction, glaciers, and seismometry are probably already provided for in existing bureaus of the United States Government, and consequently need not be drawn upon to furnish a field for a geophysical institute.

For these reasons I must, therefore, agree entirely with your opinion, sir, that:

"The time seems ripe for the attempt and it would appear that success must be attended by most notable contributions to pure science, as well as to the history of this planet."

With the highest respect,

O. KOHLRAUSCH,
President, Physikalisch-technische Reichsanstalt.

[*J. H. van't Hoff to Mr. Walcott. June 22, 1902.*]

CHARLOTTENBURG *June 22, 1902.*

DEAR SIR:

In reply to your letter of May 5, wherein I am honored by your consulting me on plans for research in geophysics, I express as my conviction that an investigation in that direction might prove of the highest value, if made in a systematic way and continued for some years.

The special problem, which, I mean, deserves attention. is the physical chemistry of high temperatures applied to the chief constituents of the earth's crust, silicates in the first instance. To express myself more clearly, I add that two great problems concerning geophysics may in the present state of our knowledge be solved, viz., the evaporation of complex solutions, which have produced systemic deposits. such as salt layers, etc., and secondly, the cooling down of molten masses, that have produced the volcanic and plutonic formations.

With the first problem, by far the easier one as regards apparatus, etc., I have been occupied for more than six years, and a series of twenty-six publications in the Annals of the Prussian Academy of Sciences (1897–1902) shows how far these researches have been carried out.

By the same post I send two abstracts. one by Armstsong, the other forming part of the lectures I delivered at Chicago last summer. It is my opinion that, guided by the indications obtained, the second problem, concerning the formation of plutonic and volcanic products. may be successfully taken up, but to pursue it in a systematic way the co-operation of different forces, furnished with special facilities for research extending over some years, is needed, such as can only be realized by an Institution like that newly founded by Mr. Carnegie. I may add, however, in favor of the project, that when once an installation for high temperature research, with the special aim pointed out, has been established, many problems of the highest importance might be successfully studied with the same means. I suggest, moreover, that the use of Niagara Falls as a source of electric heat for the above purpose be taken into consideration.

Respectfully yours,

J. H. VAN'T HOFF.

[*Prof. G. H. Darwin to Geo. F. Becker, June 26, 1902.*]

NEWNHAM GRANGE,
CAMBRIDGE, ENGLAND, *June 26, 1902.*

MY DEAR SIR:

It is clear that there is a very wide scope for good work in geophysics in all the directions which you specify in your memorandum, and, as I said to Mr. Walcott, the limitations are set rather by the men than by the subjects.

I do not know the procedure by which the Carnegie trustees will allot the money to various projects. It may be necessary to draw up a scheme. complete in all lines of research. Mistakes will inevitably be made, and all that can be done to avoid them is to take great pains in drawing up the proposals.

I believe, however, that the most efficient plan would be to make the start in a humbler scale, but in such a way as will easily allow of expansion in various directions This conception would only be best if money will be forthcoming for expansions when they shall be seen to be desirable and feasible. My reasons for saying this is that a geophysical observatory and laboratory is a new thing, and can not be planned with the same completeness as is possible in the case of astronomy.

Whatever line is taken it is very desirable that you should have thorough knowledge of the methods pursued at Strassburg, Göttingen, the Italian and Japanese observatories. I would recommend that you should learn what Milne is doing in the Isle of Wight, and hear what he has to say as to equipment. I believe that Urechert's great pendulum at Göttingen is better than any other instrument of the kind for a fixed permanent observatory, but it must have been so expensive as to be beyond the means of almost any private person.

I venture to suggest two researches which would, I think, be of interest. I should like to see made a study of earth tremors and deflections of the vertical deep down in mines. I conjecture that it would be necessary to install two, or even three, instruments at the top and bottom, and, perhaps, at an intermediate depth. If this matter has been studied at all, at least it has been very imperfectly investigated. Might it not perhaps throw some light on the broad yielding of the solid earth?

The expense would no doubt be considerable, and the observer must be a competent man who can make daily visits to the bottom.

I do not think we yet know sufficiently how far neighboring instruments give consistent readings even to the horizontal plane, still less the nature of the differences in the vertical line. There is always a doubt as to the proportion of the observed deflections which are due to mere local warping of the soil and building. For example, it seemed useless for my brother and me to go on with our pendulum observations here, when a large part of what we noted was probably merely due to variations of water level in the river gravel. Consistent readings from two instruments several miles apart on the chalk hills would have had a very scientific value.

If, however, we wish for example to study changes in the vertical, to prove the existence or nonexistence of tremors due let me say to distant volcanic explosions, etc., we require platforms not affected by changes in temperature, underground moisture, and beyond the range of artificially produced vibrations. Stations complying with conditions such as these are rare. But would it be wise to build and equip an institution in a proper locality before preliminary investigations showed the realty of the phenomena to be investigated?

Then, again, there are so many researches where Mahomet must go to the mountain. Take our seismic survey: With my stable as a laboratory and the co-operation of thirty-six stations distributed over the world, you know the results we are obtaining respecting the physical nature of our planet, the districts which are yielding in its crust, etc. Strassburg with its Government support and a laboratory I envy, is without this outside co-operation, with the result that it can do but little more than publish its own registers.

Again, if we wish to make observations on seismic disturbances or changes in the vertical underground, we are again outside our four walls. To measure the effects of tidal loads on coast lines—the effects of barometrical pressures—secular deformations in the crust of the earth—the variations in magnetic elements, or changes in *g*; say in the vicinity of extinct volcanoes, we may be 1,000 miles away from our laboratory. And so I might continue suggesting lines of research, none of which could be carried out in a particular building. To my mind, what is required is a trust for geophysical work. If the funds admit, let there be a central office laboratory and staff, but the chief expenditures should be for investigation, carried out in suitable localities or at existing establishments. * * *

The enclosed pamphlets will show Mr. Walcott what I do during the year. In addition there is a heavy correspondence with stations and the instructors at Shide to be looked after.

This work costs about £150 a year * * * ."

The other subject which I suggest is a study of the actual motion in geological faults. My brother Horace has recently begun observations at a well-known fault in Dorsetshire. He has a very delicate level clamped to the rocks on the two sides of the fault and has begun his readings. Nothing has as yet been published,* as he has met with many preliminary troubles, but I think that the results should be of interest even if they are purely negative. I am sure that he would be willing to put his experience at your disposal.

To initiate a geophysical observatory even the humbler lines that I advocate for a beginning will undoubtedly prove a very arduous undertaking. If the work is entrusted to you I am sure that your great geological experience, and all the thought which you have devoted to geophysics will prove invaluable.

I look forward with the greatest interest to future developments and earnestly hope that the project will meet with the approval of the trustees.

May I ask you to show this to Mr. Walcott, who has just written to me in the same sense as you.

I remain yours very sincerely,

G. H. DARWIN.

G. F. BECKER.

P. S.—After writing the above I thought I had better consult Milne. He writes: "Very much geophysical work may be done in a laboratory, but it must not be overlooked that there is very much that can be done outside the same. Many of the hitherto suggested investigations, as for example, those relating to high temperature phenomena, might be carried out at existing laboratories, provided they had the means and the men."

[*Dr. W. Nernst to Mr. Walcott, August 26, 1902.*]

[Translation.]

GÖTTINGEN, *August 26, 1902.*

TO THE SECRETARY OF THE CARNEGIE INSTITUTION,
Washington, D. C.

DEAR SIR: I was unable to give an immediate answer to your esteemed letter of May 5 of this year, because, owing to pressure of

*See p. 119, B. A. Rept., 1900.

work during term time, I had no opportunity to occupy myself with the matter in such detail as its importance deserved.

I fully agree with you in holding that the physics of the higher temperatures in particular is a field of the greatest importance for the theory of all physical and chemical processes on the one hand, and, on the other hand particularly apt to throw new light on many questions of geophysics. Furthermore, we are here dealing with a subject which can hardly be dealt with by means of the ordinary resources of laboratories, because it demands special appliances. Of course it must be remembered that these exceptional requirements are demanded not only of the instrumental equipment, but also of the experimenter himself. in other words, a notable result can only be hoped for if the right men are found for the execution of this difficult work. Fortunately, your country possesses such investigators; in particular, you have in Professor Barus one of the foremost authorities in the field of high temperatures.

Having endeavored to answer your special inquiry, allow me, Dear Sir, to add a few remarks of a more general nature which forced themselves on my mind while reflecting on the magnificent institution which you are about to create. It seems to me that the intentions of the generous founder might, perhaps, be most fittingly carried out by the creation of an *academy*, organized on the whole in a manner similar to the academies of the Old World, but yet, in view of the abundant means at its disposal, differing from our academies in one essential point. The members of our academies, such as those of Paris, Berlin, Vienna, Göttingen, etc., are academicians only incidentally. The academic position is solely an honorary office, and in many cases hardly more than a mere decoration. Your institution, it seems to me, would be in a position to establish an academy constituted of investigators of the first rank, who would be academicians and nothing else; that is to say, they would be so placed as to live exclusively for the interest of scientific investigation. My idea is that a small circle of the most eminent investigators might gather in Washington, composed of those whose method requires a scientific laboratory, with a small outfit appropriate to their special mode of investigation. It may be remarked that the cost of these laboratories would not be great compared to the corresponding laboratories of universities, because in the latter the larger part of the funds is devoted to instruction, not to investigation, and for the same reason the proposed laboratories might for

the most part be of much smaller dimensions than laboratories for instruction. This would also be an advantage inasmuch as the efficiency of the experimenter is by no means always in proportion to the size of his laboratory. In my opinion, small laboratories, but with first class outfit, should be the aim of every investigator.

The men for such an academy could be found in your country at once in most branches, and in all branches in the near future, since your country is progressing in science at a rate hardly equaled elsewhere.

I fear these remarks far exceed the scope of the question which you addressed me. It is needless to say that I shall take pleasure in giving any further information that you may desire.

Very respectfully,

DR. W. NERNST.

REPORT OF ADVISORY COMMITTEE ON GEOGRAPHY

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: The subject of geography being named without limitation in my appointment as adviser. I wish briefly to set forth the contents of the subject as a whole, in order to indicate the relation of its several parts and to point out those that seem most in need of aid

Geography is best defined as the study of the relationship between the factors of physical environment (causes) and the responses of the environed organism (effect), and hence of these factors and responses themselves. The subject may be treated systematically or regionally, but neither of these chief divisions has yet reached a mature development. Systematically, all the different categories of physical factors and of organic responses are taken up, illustrated by typical examples, and arranged in some appropriate order. Regionally, all the physical factors and organic responses occurring in a certain region are brought together, in an arrangement corresponding to that adopted for systematic geography. Under either of these headings special attention may be given to larger or smaller parts of the subject; but at present it is too generally the case that these partial studies are undertaken without any sufficient consideration of the relation of the parts of the whole.

The Carnegie Institution can do much to aid the development of the scientific and thorough study of geography, and some practical suggestions to this end are given below; but it is desired to explain at the outset that some of the most promising results may be expected from comparatively young geographers, for the reason of the undeveloped condition of the subject as a whole. In this respect geography occupies a very different position from astronomy. Geography has long had the service of men of energy and of action; but the more philosophical side of the subject has been little developed by geographers. Astronomy, on the other hand, has for centuries commanded the services of the greatest intellects of the world, and it today is recognized as holding a leading place among the sciences. It is in the hope of developing the more scientific or philosophical side of geography that I recommend below the granting of aid not only to professional geographers of established position, but also to

students of promise; and I should greatly regret it if the undeveloped position of scientific geography resulted in its being set aside for the support of other subjects already so well endowed that large and influential committees of eminent experts are easily organized in their advocacy.

The two chief divisions of geography may be further subdivided. Physiography includes all the facts of physical environment of life on the earth; and the other half of the subject—unnamed, but fairly designated by such a term as ontography—includes all the kinds of responses of physically environed organisms. Physiography is further divided into the earth as a globe, the atmosphere, the oceans, and the lands; ontography may also certainly be subdivided, but no divisions are at present agreed upon among geographers.

The Earth as a Globe.—All problems concerned with the size and form of the earth are best left with governmental surveys. If particular problems arise in this connection, such, for example, as local magnetic surveys, they will be duly considered when applications are made by specialists. For the present, no recommendation is made under this heading.

The Atmosphere.—A. There is much need of a scientific study of the failures of weather prediction. Within the last month there have been two examples of failures of precisely the same class as those of twenty years ago; and yet it does not appear that serious research is in progress with a view to lessening such failures. Much might be done by a competent investigator who should review maps and predictions and classify successes and failures.

B. Another line of work toward the same end is a study of the movement of the upper clouds by means of the horizontal mirror. A study of this kind might be conducted at a moderate expense and useful results could be expected in a year or two.

C. Detail of weather phenomena. The meshes of the Weather Bureau net are so coarse that many smaller phenomena slip through them undetected. It has frequently been suggested that a scheme of observation by numerous voluntary observers, coördinated under state weather services, might be organized, so as to obtain a closer view of passing changes. The work done in this direction some fifteen years ago by the New England Meteorological Society, and continued for a season or two elsewhere, is well worth systematic extension over a larger area. It is to be expected that the Weather Bureau would permit the use of franked envelopes in such work; there would be a possibly large expense in providing instruments

Printing would be no small item; and discussion of results would be an important charge. At present, it may not be possible to secure just the right men for such an investigation; but if any such should offer themselves, I should favor their being well supported.

D. Investigation of the upper atmosphere by kites and balloons, kites preferred. While this kind of work is not likely to be for the present immediately useful in weather prediction, the study of the processes of the upper atmosphere is essential to reaching a full understanding of meteorological phenomena, and should be warmly encouraged. The Carnegie Institution might advisedly support the work of two qualified observers, providing them with an outfit of kites, instruments, etc.. and allowing them to move from station to station, spending a year or more at each place. I urge that Mr. A. Lawrence Rotch, Blue Hill, Mass., as well as the Chief of the Weather Bureau, be consulted on this work.

E. Kite-flying from mountain tops deserves mention apart, as it would provide information concerning unexplored atmospheric regions. Work of this kind in connection with the Harvard Observatory at Arequipa, Peru, would be particularly interesting.

F. Since the publication of Ferrel's works there has been little mathematical or physical study of meteorological phenomena in this country. It is very desirable to enlist the interest of competent mathematicians and physicists in such work, and if any promising investigator comes forward he should be encouraged.

The Ocean.—Charting and sounding may well be left to governmental expeditions. Tides are also well cared for, being of such practical importance that they command abundant support; but the Carnegie Institution might offer aid in supplying special instruments to exploring parties; for example, sounding apparatus to some yachtsman who should undertake minute soundings of the submerged channels off our Atlantic and Pacific coasts—a very interesting and little-studied field. The actual movement of deep ocean waters should be determined, and ingenious instruments might be constructed for this purpose. Such studies well deserve aid.

The Lands.—A. The expenses of exploring expeditions had best be left, as a rule, to governmental and individual funds. There is no region, except one, whose exploration promises to produce new classes of facts of sufficient importance to call for large support; but it would be well to offer aid to well-trained explorers by supplying them with instruments, after the fashion of the Royal Geographical Society. Sextants, barometers, thermometers, plane tables, cam-

eras, phonographs, etc., as well as apparatus for collecting and cases for shipping natural history specimens, would be of great service. The announcement that the Carnegie Institution stands ready to receive applications for aid from travelers properly trained for their work would, I believe, greatly promote the development of scientific travel among our young men. I would, however, urge that emphasis be laid on "proper preparation" in order to distinguish scientific work in some one of the various phases of geography from mere traveling and big-game shooting

B. The exception above noted is in the Antarctic regions. New classes of facts may be expected from the study of far-southern latitudes. * * *

C. There are various special topics in which a trained investigator is sure of interesting results. For example (as suggested by Mr. Gannett), a sum might well be allotted in aid of studies of North American glaciers. Systematic work on glaciers should be inaugurated, to be continued for a long period of years. A moderate sum would be of much service in enlisting the interest of travelers, sportsmen, and others, who could bring good results home by following systematic instructions. The special studies of shore lines need extension in the field. Similarly studies of Appalachian rivers offer results of value in several connections; but the most available men are already supported in such work by the United States Geological Survey. It should be noted that in this sort of work it is not necessary, not even desirable, to explore in the ordinary sense—that is, to go into previously unknown regions. There is abundant field for scientific geographical exploration east of the Mississippi, and still more west of it.

Probably the best way to encourage work of this kind would be to announce the establishment of five or ten fellowships in geography, of from $300 to $1,000 each, to be assigned to well-prepared students who wish to undertake investigations in some part of the broad geographical field. It would be my preference that the idea of relationship, indicated above, should be made a prominent part of all such studies, in order to bring forward that fundamental principle that underlies all true geographical study. The work might be systematic—that is, concerning some special subdivision of the subject, as glaciers or shorelines, above mentioned, or it might be regional, concerning all the geographical features, inorganic and organic, of a certain region, Pennsylvania, for example. A study of this kind, well conducted, would greatly enlighten the public, even the scien-

tific public, as to the true nature and content of geography. As has already been pointed out, the principles of geography are so little developed that it will be necessary to begin studies of this kind in relatively unadvanced stages, for the advanced stages are yet to be reached Some of these studies might be well undertaken in the library, at least in part, for there is already on record a large amount of material whose full discussion and digestion still requires much patient labor.

Summary.—In view of the foregoing suggestions, I make the following recommendations

A. Announce the establishment of ten fellowships of from $500 to $1,000 each, offered annually for aid of special investigations by well-qualified students. The subjects of these investigations might be taken from any one of the subdivisions of geography. Some of them are specified above.

B. It should be further announced that the Carnegie Institution stands ready to aid well-qualified travelers and explorers by lending them instruments, etc.

C. Subjects of investigation above noted, apart from those just referred to, should not be advertised. They are mentioned in this report in order to indicate the character of work worthy of support; but it seems best to wait till the right man comes forward before undertaking them.

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Respectfully submitted.

W. M. DAVIS, *Chairman.*

HARVARD UNIVERSITY, *April 12, 1902.*

REPORT OF ADVISORY COMMITTEE ON METEOROLOGY

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: The problems of future research in meteorology can be best appreciated after considering the past history of our knowledge of this subject.

The ancient history of meteorology ends with the establishment of a system of observing stations in southern Europe in 1653 by the Grand Duke of Tuscany. These stations were supplied with thermometers, barometers, hygrometers and wind vanes. From that time until the establishment of the Mannheim society, about 1780, local climatology was the principal study, but that society organized a system that was intended to cover the world as far as possible, and stimulate the study of the atmosphere as a whole. The observations published by it afforded Brandes in 1820 an opportunity to chart the atmospheric conditions for Europe day by day; this was the beginning of the study of storms and local atmospheric movements in Europe. About the same time Redfield began collecting and collating the logs of vessels at sea, and laid the foundation of our knowledge of Atlantic ocean hurricanes. Very shortly after this, Espy began collecting and charting observations in the United States and Canada for the study of the characteristics of tornadoes and the general rains of this country. In 1867 Leverrier began publishing his charts of Europe, America and the Atlantic Ocean under the title of the International Atlas. In 1875 General A. J. Myer began the publication of the Signal Service Bulletin of International Meteorological Observations, with an accompanying atlas, showing the conditions over the whole northern hemisphere, for each day of the years 1875–1884; this was continued as a monthly summary until 1889. Up to this time observations had been restricted generally to the earth's surface and sea level, except for an occasional balloon ascension and the regular observations of the clouds. In 1790 Alexander Wilson at Edinburgh, and in 1882 E. D. Archibald at London, used kites to get temperatures and wind velocities at considerable altitudes. About 1885 Mr. William Eddy, who had become famous for the use of kites to carry heavy objects high in the air, was asked by me to turn his attention toward the application of the kite to meteorology by way of carrying up self-recording apparatus. Since that time this use of the kite has

been especially developed by Mr. Rotch at Blue Hill and Professor Marvin at the Weather Bureau, so that finally, in 1899, Professor Willis L. Moore, as Chief of the Weather Bureau, was able to organize a system of seventeen kite stations and lay the foundation for compiling charts of temperature, pressure, and wind at an altitude of one mile above sea level. In 1897 Monsieur Leon Teisserenc de Bort began at Trappes, near Paris, to systematically send up small balloons filled with hydrogen, carrying self-registering apparatus to great heights, and this work is now carried on at ten or twelve stations in Europe, so that by means of simultaneous ascensions we are able to make maps of the condition of the atmosphere at an altitude of 10,000 or 15,000 meters above that portion of the world.

With regard to the ocean we understand that at the present time the Hydrographic Office of the U. S. Navy, the Seewarte at Hamburg, and the Meteorological Office at London are compiling not only monthly and annual summaries for each square degree of the ocean surface, but also daily maps of the atmospheric condition over those special parts of the ocean from which a sufficient number of observations can be obtained. Daily maps of the Atlantic Ocean have been published by the British and Danish meteorological offices. and similar maps of the monsoon area by the Indian Government.

Although there may be approximately 25,000 meteorological stations on land at the present time, and as many more good observers at sea, yet a large part of both the land and water surfaces are still unrepresented by charts or observers. Consequently our knowledge of local climates is very unsatisfactory, and our knowledge of the local movements or the so called physics of the atmosphere has only just begun to be developed.

Of course meteorological research must cover an extensive field, including all lands and seas and extending upward to an elevation in the atmosphere as yet undetermined, but beyond which we may speak of cosmic physics or the physics of the ether. It also includes the applications to the atmosphere of our knowledge of hydro-dynamics thermo-dynamics, kinetic theories of diffusion, optics electricity, all of which are generally included in the term physics of the atmosphere. The study of local and general climatology affords also a practical illustration of the mathematical laws of probability.

The applications of meteorology to the practical needs of mankind and the relations of meteorology to biology, geology and other

branches of science are numerous and important. but the researches along these lines are outside of the fundamental science of meteorology.

On account of these important applications every civilized nation maintains a meteorological office and service responding to the practical needs of the people. At the present time there are about thirty larger organizations of this kind and twenty smaller. The progress of the science of meteorology, as distinguished from its practical applications, is largely but by no means exclusively in the hands of these national organizations. This science has also been fostered by meteorological societies of which the most prominent are those of France, England, Scotland, the Mauritius, Austria and Germany, to say nothing of the societies of Mannheim and New England, now defunct. Nearly every general scientific society also encourages meteorology. The universities of the world have in some cases organized and given special attention to meteorology; notable among these are the professorships held by Kämtz, at Dorpat; Woeikof, at St. Petersburg; Schmid, at Jena; von Bezold, at Berlin; Hergesell, at Strasburg; Lamont, at Munich; Hann and Pernter, at Vienna: Mascart, Angot and Brillouin, at Paris; Wm. M. Davis and R. De C. Ward, at Harvard. In addition to these full professorships, there are many instructors and lecturers, repsenting minor courses, in which climatology is taught as a part of the education of a physician, an engineer or a geologist.

The number of students who take the higher meteorological courses, and especially those who make meteorology the major subject for the attainment of the degree of Ph. D., is very small; apparently it does not average more than five per annum for the whole world. This condition of affairs is quite remarkable in consideration of the great importance of the science, and is to be explained, partly, by reason of the difficulty of the subject, but principally by the fact that the needs of the meteorological services of the world have, as yet, not been properly made known to the universities and to those who provide for the support of the faculties. As a consequence the older and prominent professional meteorologists are those who originally made a special study of chemistry, or astronomy, physics, navigation, or engineering; this gives to each meteorologist a tendency to prosecute meteorological studies along certain pre-determined lines of thought. There are, however, a few cases in which most important work has been accomplished by those who have approached the subject from the point of view of math-

ematics and analytical mechanics, and it is along this line of investigation that we must expect the most important discoveries in the future. No matter how diligently we prosecute our observations and collect and study the observed data, yet it must always be true that the fundamental laws controlling atmospheric phenomena must be those of mechanics and must be investigated by those skilled in mathematics. Meteorology has attained a status analogous to that of astronomy in the century between Newton and La Place. It is ready to receive a new leader and is looking for him. A hundred experimentalists and thousands of observers are perfecting the date of observation, but the crying need is for one who shall elucidate our complex phenomena to the satisfaction of the students of mechanics.

In this search for men and the pre-eminent right man, meteorologists welcome the assistance of the Carnegie Institution, and your committee would respectfully submit the following general recommendations which will be supplemented by fuller details whenever called for:

1. Meteorology should be treated by you as a very broad subject, always embracing the atmosphere as a whole. The Institution may leave it to local observers to investigate the climatology of their respective localities, embracing only one-tenth of the surface of the whole globe: the remaining nine-tenths, including the polar regions and the high seas, are open to your investigation without exciting international jealousies or questions of propriety.

Meteorology should be treated as a part of terrestrial physics, the other branches of which will be terrestrial magnetism, oceanography, geology, vulcanology, seismology, and similar matters that affect the globe as a whole. It, as it is reasonable to hope, the Institution organizes a department of research in terrestrial physics, then we recommend that meteorology be given a prominent place or division therein, and that the division be conducted by three persons, namely, a mathematician, an experimental physicist, and a bibliographer.

2. It is reasonably certain that the young men, who from year to year attain the degree of Ph. D., do in some cases desire to devote themselves to meteorology, but at the present time so little inducement is offered to men of high scientific talents to devote themselves to this field of work that, as a matter of necessity, most of them seek employment elsewhere. The Carnegie Institution will do its best work for meteorology by securing the services of young men who have shown already a genius for investigation in mathe-

matics, mechanics, and physics. Such persons should be appointed Fellows in Meteorology, with salaries of from $1,500 to $3,000, depending on age and experience, renewable from time to time, as occasion may demand, and who shall devote their whole time to appropriate research. The general trend of their researches should be prescribed by the three older persons who conduct this division.

3. A laboratory arranged for general physical research should be at the disposal of the fellows in meteorology, but at other times a so-called meteorological observatory, or special laboratory, would be needed, built with a view to special investigation. In some cases the fellows would be obliged to occupy distant stations or to take ocean voyages or aerial voyages, or to make use of sounding balloons and kites. All this does not constitute a very expensive matter, as the physical laboratory must be provided for general physics, and the meteorological laboratory or observatory is quite a simple matter.

4. The research problems appropriate to the Carnegie Institution may in some cases seem also appropriate to the various government weather bureaus, but that is principally because such bureaus have not confined themselves to the practical applications of what is known in meteorology, but have also devoted a small portion of their attention to research in lines that promise to be helpful to the progress of their specific duties. Without encroaching upon the privileges and duties, or the fields of labor imposed upon these government bureaus, the Carnegie Institution may often prosecute studies along the same lines of inquiry, and, in fact, frequently this will be very desirable, especially when a given investigation requires the co-operation of more stations or more individuals than any one bureau can command. Several large works of this character have been suggested by Professor Willis L. Moore in his communication of April 15, as Chief of the United States Weather Bureau, and we repeat them herewith, after rearranging them in what we conceive to be the order of their importance to the immediate needs of meteorology. Whenever the Carnegie Institution intimates its desire and intention of taking up any one of these works, we have reason to believe that the Weather Bureau, the Hydrographic Office, and other similar institutions in Europe will co-operate most heartily. The list is as follows:

(a) General bibliography of meteorology up to 1900 inclusive.

(b) General meteorology of the upper atmosphere, to be studied by means of clouds, balloons, kites, mountain stations, polarization of skylight, or any other method of observing the upper air.

(c) General meteorology of those parts of the ocean not already provided for.

(d) Daily weather maps of the world in general, compiled by international co-operation, from reports received by mail from observations on land and at sea.

(e) The relations of meteorology to terrestrial magnetism, atmospheric electricity and solar radiation, including the absorption of sunshine by the air.

5. Among the minor subjects that may be taken up by single individuals in the physical laboratory or meteorological observatory are the following:

(f) All problems relating to thermometry, barometry, actinometry, anemometry, hygrometry, pluviometry, nephelometry and other branches of instrumental work. In every field of observation we need continuous self-registering apparatus more sensitive, more delicate and more reliable than we at present have.

(g) Experimental laboratory methods should be devised to elucidate the physical processes of the formation of cloud, fog, rain, dew, frost, snow and hail, which work will necessarily be a continuation of that already done by Carl Barus and C. T. R. Wilson, so far as concerns clouds and rain, and that done by Mr. W. A. Bentley, of Jericho, Vermont, as far as concerns the microphotographs of snow crystals.

6. But, as I have said, all these observational researches must be supplemented by mathematical work on the dynamics of the earth's atmosphere. Only a few elementary problems under this category have as yet been solved satisfactorily, and perhaps those that remain cannot be solved until new branches of mathematical analysis shall have been developed for this purpose. It is in this line of work that we most earnestly anticipate the assistance of the Carnegie Institution. A few years ago meteorologists were encouraged to find that von Helmholtz had turned his attention in our direction. But his death in the prime of life crushed our hopes. At the present time there are several prominent workers on the mechanics of the atmosphere, such as Bigelow in America, Bjerknes and Ekholm in Sweden, Moeller, Sprung, Wien, Pockels and von Bezold in Germany, Marchi in Italy, Pernter and Margules in Austria, Diro Kitao in Japan, and Brillouin in Paris. These all combine a good practical

knowledge of weather maps and actual meteorology, with a knowledge of the present status of mathematics But it is evident that mathematical analysis is as yet scarcely able to cope with the real problems of meteorology. These authors have therefore generally treated only the simplified problems idealized from nature, whereas apparently we need mathematicians of the highest genius, who shall devise new methods applicable to the complex conditions that control the atmosphere. It is such a mathematician as this that we had in mind in recommending that three persons, of whom the mathematician should be chief, be entrusted with the conduct of the division of meteorology.

Respectfully submitted,

CLEVELAND ABBE

Chairman.

JULY 14, 1902.

MAJORITY REPORT OF ADVISORY COMMITTEE ON CHEMISTRY.

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: The Advisory Committee on Chemistry appointed by you, consisting of the undersigned, have given their earnest attention to the subject submitted to them. They have kept clearly in mind that the main object of the Institution is to encourage original research. The problem is to determine what methods of expending money will be most likely to accomplish this main object.

In the first place, it appears clear to your committee that, so far as Chemistry is concerned, the most valuable researches are to be looked for in the universities, colleges, and technical schools. While they are well aware that researches in the field of Chemistry are carried on in other laboratories than those of the universities,* they are of the opinion that the university atmosphere is the most favorable for such work and, as a matter of fact, for many years past the advancement of Chemistry has been largely due to the work done in university laboratories. We must, then, look to the universities for the men and for the conditions if our object is to further original research. Anything that will tend to increase the efficiency of the men already engaged in research work will be helpful. This efficiency can be increased in two ways:

(1.) By relieving the men from a part of the routine work they are now doing.

(2.) By placing research assistants at their disposal, and by supplying them with books, apparatus, and material.

The committee agree that the second way would undoubtedly lead to good results if followed carefully. They do not agree that it would be wise to follow the first way, at least with the aid of funds furnished by the Carnegie Institution.

In accordance with these general ideas, the committee agree upon the following articles of belief:

1. The best research work is likely to be done under the auspices of the universities.

* In this report the term university is used in the larger sense, as including all educational institutions in which research work is carried on.

2. Some teaching is helpful, and therefore desirable, in connection with research work.

3. Those should be encouraged who have shown their power by independent research work and have shown persistence under difficulties.

4. One of the obstacles in the way of research work in Chemistry in this country is the large amount of routine work that some of our best men are required to do.

5. Good work is most likely to be done as a result of individual initiative.

We therefore recommend, as suited to the needs of chemical research, the establishment of a number of *Carnegie Research Assistantships*, the assistants to be appointed thus.

A number of chemists now carrying on research work, who have given clear evidence that they will continue to carry on such work, are to be selected as worthy of the aid of such assistantships. They are to have power to appoint their own assistants, under such conditions of time and compensation as may be acceptable to the Board of Trustees of the Carnegie Institution.

We recommend, further, that workers should, when necessary, be aided by appropriations for the purchase of apparatus, material and books. In such cases the applicant should make a clear, but not necessarily detailed statement in regard to the character of the work to be done and the kind of apparatus needed.

Respectfully submitted.

IRA REMSEN, *Chairman*,
T. W. RICHARDS,
E. F. SMITH,
Committee.

OCTOBER 14, 1902.

MINORITY REPORT OF ADVISORY COMMITTEE ON CHEMISTRY.

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN. While approving the plan recommended in the preceding report, I feel that it is only a partial remedy for the difficulties which retard American research. To me the above-named *first* method of increasing the efficiency of the men already engaged in research seems a more important means, namely, the plan *to relieve university professors from a part of the routine work which they are now doing.*

It is true that in a few great universities some at least of the professors are given time for research. These men are the chief examples of the value of the university atmosphere. But this freedom is the exception rather than the rule Not only are most American professors overburdened with routine work, but nearly all are obliged, by the inadequacy of their salaries, to consume time and energy in hack work, done merely to obtain a competency. Neither money for assistants nor money for materials can penetrate to the root of this trouble. Such money might, indeed, prove but a temptation to overwork and consequent break down.

It seems to me that *the difficulty can be most satisfactorily overcome by the establishment of Carnegie Research Professorships*, to be awarded to those whose past originality and persistence have shown that they deserve such positions. These men should be allowed to retain some teaching work in the university of their choice, and should be paid for such work by the university, but the bulk of their salary should come from the Carnegie Institution. This arrangement would cause each Foundation to pay for its appropriate work.

The appointments to these professorships might be made for a definite term of years, subject to reappointment in case the appointee has given evidence of due earnestness and success; or they might be terminated only by death or proved inefficiency. The total salary should be large enough to relieve the professor from pecuniary worry, and he, on the other hand, should promise to engage in no money making pursuit outside of the university. It would be well to provide each professorship with a Carnegie Research Assistantship.

The *nominations* to these professorships of chemistry might be made by a disinterested foreign committee of experts, such as J. H. van t Hoff, E. Fischer, W Ostwald, M Berthelot, and W. Ramsay; or else the candidates might be selected by the independent voting of a large number of the leading American chemist Of course the *appointment* would rest with the Trustees.

This idea is not wholly new. The German Government has already begun to establish somewhat similar professorships, and unless America does likewise there is danger of our dropping yet farther behind.

It seems to me that the establishment of these professorships would not only benefit science through the appointees but would also furnish an immense stimulus to the prosecution of research among younger men. At present an intelligent and far sighted man perceives that he cannot hope to provide comfortably for a family if he gives his chief energy to research. There is no prospect in that direction. The able teacher or administrative officer in a college may become president, the able inventor may secure a competency through his patents, but the pure investigator is doomed to poverty. There is no doubt that this lack of prospective advancement has driven many a brilliant American away from the vastly important field of activity which it is the office of the Carnegie Institution to foster.

Most respectfully submitted.

T. W. RICHARDS.

OCTOBER 14, 1902.

REPORT OF ADVISORY COMMITTEE ON ASTRONOMY

CONTENTS.

APPENDICES.

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: Out of the multitude of problems in general and in detail which your Committee in its advisory capacity has studied with care, the importance of certain general views concerning astronomical interests has impressed itself on our minds. We think we ought to state some of the most important of these at the outset, because of their general application to all recommendations which we, as advisers, desire to present for the consideration of the Carnegie Institution.

SUPPORT OF ASTRONOMY IN THE UNITED STATES.

The first point (intimately connected with the second) is that the relatively small support which American astronomy receives from the general and state governments contributes, in our opinion, a strong argument for a more liberal support to astronomy on the part of the Carnegie Institution than might be properly urged were the circumstances otherwise. This applies both to the character and amount of support which is needed. The leading governments of Europe each maintain in whole, or in part, many astronomical observatories distributed at various places in their respective dominions. The government of the United States contributes to the support of a physical observatory in connection with the Smithsonian Institution. It also maintains what was once known as the National, and subsequently as the Naval Observatory, in charge of officers of the Navy. One of the objects of this institution is to make astronomical observations. The general government maintains no observatory outside of Washington. Several observatories have been established by state governments in connection with their respective universities, to which more specific allusion will presently be made.

NEED FOR MORE WORKERS.

The second point to which we invite special attention is that the first need of astronomy in this country as it seems to us, is not for more buildings and instruments so much as for more astronomical workers to use the appliances which are already provided. In the observatories which have been established with or without state aid, the liberality of private individuals in providing for buildings and equipment has had no parallel in any other country. We have nearly as many large telescopes as exist in all other countries of the world combined. Included among these are the telescopes at Yerkes Observatory (40-inch) and at Lick Observatory (36-inch), which are the largest in existence. On the other hand, permit us to call attention to the remarkable fact, that, with one exception among state observatories, and two exceptions among those established and supported by private endeavor, none is maintained with an income which is much larger than is necessary for the payment of incidental expenses, together with the salary of the professor in charge, whose teaching duties usually leave him little opportunity for the routine

part of research. Thus the American system is weak in its most vital part. Problems in great number are pressing for solution; there is already a generous provision of observatories and equipment inviting use; there is a body of skilled investigators who are anxious to make good use of these material facilities: but there is very marked deficiency in means for their support. By support is meant the necessary provision for assistants and computers and for other expenses incident to the maintenance of active research on important problems. In reference to almost all the important subjects of inquiry, research in astronomy is attended with the necessity for a very great amount of skilled labor in measurement and computations. The conception of new ideas in investigation, the formulation of plans and methods, invention of devices to improve and shorten labor, and the deduction of results demand the same order of ability that is required everywhere in the direction of work in exact science; but usually in the details the same kind of measurement or computation has to be repeated in the same way thousands of times, sometimes on thousands of different objects, requiring for success the habit of accuracy, or manual dexterity, quickness and industry, rather than a high order of scientific ability. We believe that skilled computers and observers for the routine work are needed in greater proportion for the successful prosecution of research in astronomy than in any other department of exact science; and provision for these we believe to be the most pressing need of American astronomy at the present time.

Proposal for a Southern Observatory.

The third point which has specially impressed itself upon our attention is the great deficiency of observatories in the southern hemisphere. The chief contributions of observations upon the southern sky are now coming from four or five observatories only, in a less degree from three or four others. Against this we have in effective force about ten times as many working observatories in the northern hemisphere. Since more than one-quarter of the entire celestial sphere is efficiently reached only from the southern hemisphere, it is obvious that there is now very great disparity of astronomical resources to the disadvantage of the southern hemisphere. In studying this matter we have become more and more impressed with the idea that, if possible, something ought to be done to remedy this disparity. The scheme for an observatory in the southern

hemisphere presented in Appendix A may not be realized in full for many years to come, but we have thought that it may be possible to make the preliminary studies for its location and scope now, and we respectfully submit the question for the consideration of the Carnegie Institution whether it may not be also soon possible to make a modest beginning in the actual establishment of such an observatory. We regard this question to be exceeded in importance only by the urgent need of provision for current work to which we have already alluded.

Coöperation in Research.

We think that the Carnegie Institution might do well to encourage a greater degree of cooperation among astronomers, and we believe that its organization and scope would be well suited to this purpose. There are large fields of astronomical work involving masses of observations and computation which cannot be successfully dealt with by any single existing observatory. Numerous examples of effective coöperation upon extensive plans of observation are to be found in recent astronomical work. The catalogues of stars prepared by various observatories under the auspices of the Astronomische Gesellschaft represent a case of this kind. In order to produce the most valuable results and to escape the dead level of mediocrity, organization must leave room for the exercise of individual genius.

Problems for Research.

In the following suggestions regarding certain lines of research which we believe could be wisely aided by the Carnegie Institution, we have done little more than enumerate specific problems. For details we would especially refer to the appended individual reports of members of the Committee.

The work of astronomy may be broadly classed under two heads: (1) Investigations which involve the determination of the positions of the heavenly bodies and of their motions of every kind, real and apparent; (2) investigations on the physical and chemical constitution of celestial objects.

The Sidereal System.—Fundamental determinations of star positions lie at the foundation of the science. It is of the first importance to know the exact positions and motions of the brighter stars, since all other observations for the positions of celestial bodies are

based upon this knowledge. There is decided deficiency in determinations of the positions of the principal stars at the present time. This work is very exacting and laborious, and, in our opinion, is worthy of ample aid. The extension of such observations to the southern hemisphere deserves the careful attention of the Institution.

Much work still remains to be done in the deduction upon modern principles of revised results from the older series of observations. This work is of very great importance and is worthy of support.

Measurement of the parallaxes of stars to ascertain their distances from the earth should be undertaken with powerful instrumental means on broader lines than those hitherto followed. On the results of the investigations mentioned in what precedes, combined with those derived from the spectroscopic measurement of stellar motions in the line of sight, and the determination of the positions of nebulæ, the solution of the great problem of the structure of the universe must be based.

The accuracy of star positions is connected with an exact knowledge of the constant of aberration, which demands further study by new and more perfect methods. This constant has also a physical interest of its own. Equally desirable is more precise information regarding the changes in the direction of the earth's axis of rotation, rendered possible through observations of the variation of latitude

Many problems relating to the stellar system call for research. The existing uncertainty regarding the motion of the solar system in space should be remedied through a more comprehensive investigation than has yet been attempted, an undertaking which is now especially timely and feasible.

The Solar System.—The lunar theory requires special attention. Tables in general use are based upon Hansen's investigations published forty-five years ago, and need thorough reconstruction in order to meet the present requirements of astronomy. Of fundamental interest is the question whether Newton's law of attraction, according to inverse squares, is exactly true within the limits of error of observation. It is desirable that this problem should receive further investigation. * * *

The minor planets, nearly 500 of which are known, present a problem of much difficulty, which demands attention. Some provision should be made for more numerous computations of their orbits. This subject can be effectively handled by means of well-devised coöperation, to which it is very well adapted.

The computation of the solar parallax from observations of the

minor planet, Eros is a subject of interest at present. A vast accumulation of photographs, together with some micrometric measurements, has been secured From lack of funds or other causes, little progress appears to have been made in rendering the results available to science. What share in this work, if any, should be undertaken by the Carnegie Institution is a matter for future consideration after a better knowledge of the situation may have been obtained.

Publication of Results —There is reason to believe that there are in existence valuable series of observations, calculations, and compilations of great interest and prospective service to science which remain in manuscript for lack of means of publication. What shall be done in this connection depends, of course, upon the general policy which the Trustees of the Institution may adopt in reference to the matter of publication in general. * * *

Astrophysical Investigations.—The principal object of astrophysical research is to ascertain the physical and chemical constitution of the heavenly bodies, and to trace out and explain the successive stages in their evolution from nebulae.

One of the most important pieces of work now required in this field is a systematic photographic survey of the nebulæ in both northern and southern heavens. Photography of the moon and planets under particularly favorable instrumental and atmospheric conditions. with the hope of recording details much smaller than those hitherto obtained in photographs, should also be encouraged.

In spectroscopy there is much to be done. The measurement of the radial velocities of the brighter stars is already well provided for, and as soon as possible this work should be extended to fainter objects in far greater numbers. The spectra of long-period variable stars, studied in connection with their light variations, offer an important field for research. It should be possible with suitable apparatus to photograph stellar spectra on the scale of our present photographs of the solar spectrum, permitting the displacement of lines due to pressure and other phenomena to be investigated. In connection with the study of stellar evolution, many new stellar spectroscopic researches are required. The spectra of nebulæ, particularly of the Andromeda and other spiral nebulæ, also should receive more attention.

The Sun, as the only star whose phenomena can be observed individually, should be thoroughly studied with the best modern appliances. The law of radiation, constitution of the absorbing

atmosphere, spectra and thermal radiation of Sun spots, distribution of faculæ and prominences in latitude and longitude, etc., require extended research. An important investigation for which provision should be made relates to the amount, nature, and possible variation of the total solar radiation.

In the study of the brightness of the stars special attention should be directed to the photometry of the fainter stars and the thorough investigation of variable stars of long period. As all absolute magnitudes are determined visually, further experimental researches are required to solve the difficulties of measuring the brightness of stars by means of photographs. The photometry of asteroids also deserves attention.

IN RELATION TO NEW INSTRUMENTS.

As we have stated in the first part of this report, we consider that additional assistants and computers are more urgently needed than new instruments; but it is, nevertheless, true that many important advances can be made as soon as certain new instruments are provided. The construction of a new instrument entails the support necessary for its maintenance in active investigation and this point must naturally receive consideration in connection with any proposed grant.

We do not feel called upon at the present time to enter upon the discussion of the ownership of instruments; this will naturally depend upon the general policy adopted by the Trustees. Buildings for housing the instruments might perhaps be provided by the institution to which they are supplied, but domes, which are so constructed that they can easily be transported from place to place, might remain the property of the Carnegie Institution, if so desired. We have thought it proper, however, to express some opinion as to the general, or ideal, desirability of a few of the more important projects for new instruments that have come to our attention. These are matters which may be worthy of study in the future, even if it should be found that they cannot be regarded as practical propositions at the present time.

Respectfully submitted.

EDWARD C. PICKERING, *Chairman.*
LEWIS BOSS,
GEORGE E. HALE,
SIMON NEWCOMB
S. P. LANGLEY,
Committee.

APPENDIX A TO REPORT OF COMMITTEE ON ASTRONOMY.

GENERAL PLAN FOR FURTHERING SPECIAL RESEARCHES IN ASTRONOMY.

In the general report which your Committee has already submitted to the Trustees of the Carnegie Institution it has endeavored to point out some of the more immediate needs of astronomy, and especially such as it has supposed could possibly be provided for with the present means of the Institution.

Your Committee has also considered the possibility of many undertakings for which, on account of the great expense involved, the Carnegie Institution might not be prepared to make provision on a broad foundation at the present time. However, we do not feel justified on this account, in a report which aims to set forth the progress and needs of astronomical research, in omitting mention of these schemes, which may be regarded as parts of a single homogeneous plan. Furthermore, as we shall endeavor to show more fully below, it is quite possible for the Carnegie Institution to make a modest beginning through the appropriation of comparatively small sums from existing funds.

The need for special undertakings in astronomy, such as are contemplated by us, arises in part from the desire to secure special conditions of atmosphere. If the atmosphere were everywhere in a perfect state of calm, with no differences of temperature except those due to increasing altitude, the telescopic images of celestial objects seen through it would be perfectly steady and distinct. Under the actual conditions of observations as we experience them, the telescopic image is in a state of rapid and incessant vibration. On rare occasions there may be improvement over the ordinary conditions, but even at best the full optical possibilities of telescopes are never realized. Fortunately, it happens that at certain points on the earth's surface the meteorological conditions are such as greatly to decrease these difficulties of seeing. Experience has shown that excellent definition is the rule rather than the exception at certain tropical or semi-tropical stations of small elevation above the sea. Indeed, a lofty mountain peak is often inferior from this point of view, though for some purposes it may offer very great, if not indispensable, advantages in that class of observations where transparency to light and heat is quite as essential as good defini-

tion. For example, the latter requirement applies with particular force to researches upon solar radiation.

So far as we know, no single point on the earth's surface unites all the advantages required in the various classes of astronomical observation. It is very probable that a carefully planned search would result in the discovery of sites which would fulfil the requirements of certain special researches more perfectly than any now known, but in any comprehensive plan of research several stations might have to be selected, each adapted to the particular purpose required

In studying these matters from time to time it has appeared to us with increasing force that the similarity of ideas which naturally suggest themselves in the consideration of each topic points to the advisability of treating them together under a single organization.

The object of this organization would be to provide, under the direction of the Trustees of the Carnegie Institution, for the accomplishment of a variety of large and important researches which require special conditions of atmosphere, latitude, or instrumental equipment not now available, by investigators whose previous work shows them to be the best qualified More specifically, this organization could accomplish three results of the first importance:

(1) Observation by precise modern methods upon objects in the far southern sky which have been wholly neglected hitherto, or insufficiently observed, in order to complete the evidence necessary for generalizations in certain lines of astronomical research.

(2) The utilization of exceptional atmospheric conditions which exist at certain points on the earth's surface, and particularly at great altitudes, for the prosecution of important investigations which can not be undertaken to advantage in the absence of such conditions.

(3) The employment in such researches of the ablest astronomers of all nationalities through the provision of necessary equipment and other facilities required for the special work which they may be prepared to undertake.

Observations in the Southern Hemisphere.

In our general report we have pointed out the need which exists of providing for special observations in the southern hemisphere upon objects which can not be reached by observatories in northern latitudes, with a view to the completion of the evidence which is absolutely necessary for the proper and effective discussion of cer-

tain general problems in astronomy. In a later portion of this Appendix we have treated this subject more fully, and we would respectfully invite the attention of the Trustees to the considerations and recommendations which we have presented under this head.

AN OBSERVING STATION FOR SOLAR RESEARCH.

In a communication which Professor Langley, in response to a request from the Secretary of the Institution, has addressed to the Committee and which we have incorporated in this Appendix, at the end, he has drawn attention to the need which now exists for research upon solar radiation and its possible variations. We are in accord with Professor Langley in believing that a great advance in our knowledge of the physics of the Sun may be anticipated from a research of at least eleven years' duration with special and powerful apparatus, located at mountain stations, presumably in a subtropical region, substantially as he has presented the subject. He estimates that the entire cost of the undertaking would be about $500,000, and the detailed estimates which he has presented seem to warrant that view. The Sun, as the source of those energies which govern the kinetic and vital conditions of the several planets, possesses an unusual interest for mankind. As the only star whose physical condition can be investigated in detail, it offers almost the only opportunity we have for finding the key to the interpretation of problems concerning the evolution and present condition of stars presented through the evidence of the spectroscope.

As will be seen, Professor Langley, in his communication, is impressed with the utilitarian advantage which might possibly grow out of this research. He thinks it possible that by means of this study we may hereafter be able to predict climatic changes of great importance to the welfare of mankind.

We think that the scheme outlined by Professor Langley is worthy of close attention and detailed examination on the part of the Institution.

DEVELOPMENT OF A LARGE REFLECTOR.

The remarkable development of the reflecting telescope in recent years opens up a large field of possible accomplishment through the use of powerful reflectors. The photographs of nebulæ which were made with the Crossley three-foot reflector at the Lick Observatory

by the late Professor Keeler constituted a remarkable advance upon all previous accomplishment in this line. This advance has been still further signalized (according to the hearty, concurrent testimony of a very large number of specialists and leading astronomers) through the recent wonderful photographs of the nebula surrounding the new star in Perseus, and of other nebulæ, obtained by the aid of a two-foot reflector constructed and manipulated by Mr. Ritchey, of the Yerkes Observatory, under the superintendence of Professor Hale. In the latter case there is no doubt that the remarkable results achieved were due not only to the excellence of the two foot mirror itself, but also in no slight degree to the perfection of the mounting devised at the Yerkes Observatory. Indeed, it appears probable that a five-foot reflector could be constructed which would permit of the examination of the spectra of the brightest stars under a dispersion nearly or quite equal to that which is ordinarily employed in spectroscopic researches upon the Sun, and under conditions as to the use of the spectroscope itself more favorable than has ever before been enjoyed in this class of researches. We believe that if a large reflector constructed on the general plan devised at the Yerkes Observatory could be used at a carefully selected station at some elevated point in southern California, or at some place offering equal advantages in atmospheric conditions, it would be possible to surpass all which has been accomplished hitherto in the photography of nebulæ, in the measurement of motions in the line of sight of the fainter stars, in the precise spectroscopy of typical stars, and in the measurement of heat emanating from the stars. All of the researches would be of the most vital consequence to the progress of astronomy in the lines in which the astrophysical branch of that science is now tending ; and should the means become available, we believe that the prospect of success in the direction indicated would be well worthy of the attempt. In a general way we estimate that the installation of such a plant might cost from $100,000 to $150,000, and that it could be maintained in efficient operation by the expenditure of from $10,000 to $20,000 per year, according to extent of operations.

NEED FOR LARGER RESOURCES IN ASTRONOMY.

We have presented these three illustrations of attempts which it is ideally desirable to make in an effort to secure, in each instance, a unique and remarkable advance in astronomical research. They

are such as readily occur to us in a brief and summary consideration of the present needs of astronomy. No doubt other illustrations of equal importance could be presented by a more careful study of the various problems now presented for solution, and others will certainly present themselves from time to time in the future. It will be seen that in each of the three propositions we have presented it is designed to occupy fields of activity now practically vacant, in answer to an existing demand. In all cases we think the choice of new activities should be limited in this way—always as a response to some need which has naturally and demonstrably arisen out of the development of astronomical progress.

More Careful Study of the Needs of Astronomy Required.

We have presented these suggestions of plans for the extension of astronomical progress as desirable in the abstract, or ideal, point of view, and we realize that before they could be entered upon as practical propositions, in relation to the means which would be required for their actual exploitation, it would be necessary that they should be examined in much greater detail as to methods of procedure and probable cost than has been possible to us in the brief period which has intervened since our appointment as advisers. Furthermore, we do not consider that the great labor of such detailed examination would be warranted unless the Trustees of the Institution shall have signified their desire to have a more specific presentation of the various subjects. Should it seem desirable on the part of the Trustees that the larger needs of astronomy (of a nature such as we have suggested) should receive more comprehensive investigation and accurate presentation, we are of the opinion that a special commission should be duly authorized to carry out this wish.

If such a commission should be appointed it would be desirable that a sum not exceeding $10,000 should be appropriated to enable the commission to conduct such inquiries as they may deem necessary, including the examination of sites in the southern hemisphere and at great altitudes, the preparation of preliminary sketch plans and specifications for buildings and instruments, etc.

Before leaving the general consideration of this subject we wish to emphasize again the idea which we have stated in the outset, that such an inquiry, if undertaken, should be strictly limited to that class of astronomical investigations which demand special con-

ditions of atmosphere, elevation, latitude, or instrumental equipment. We believe that under no circumstances should there be any interference with present activities. For all work not requiring new and special conditions we think that progress can be most advantageously realized in coöperation with existing institutions, according to the policy already foreshadowed in the preliminaries of the organization of the Institution. Furthermore, we desire to reiterate our view, expressed in the general report and elsewhere, that it is not worth while to give serious attention to plans of investigation for which there is not a good prospect that the services for direction and superintendence of a competent and experienced investigator can be found.

Inasmuch as we have conjectured that it might be possible for the Institution to undertake the establishment of a southern observatory on a modest scale at least, we present our views upon this subject in somewhat greater detail. Following this Professor Langley presents his views regarding an observing station for solar investigations.*

An Observing Station in the Southern Hemisphere.

Observations Needed in the Southern Hemisphere.—In reviewing the needs of astronomy, those which arise from the insufficiency of astronomical observations which have been made at observatories south of the equator are found to be very striking indeed.

This deficiency on the part of the southern hemisphere extends to nearly all classes of observations of precision. It is especially marked as to the more precise researches in astrophysics.

Owing to the prospective absorption for many years of many of the leading southern observatories in carrying on the astrophotographic survey, there is little prospect that the disparity of southern observations will soon find a remedy on the part of these observatories.

Meanwhile many lines of investigations must seriously suffer for the want of needed observations upon objects in the far southern sky.

We proceed to enumerate some of the most pressing needs for work in the southern hemisphere at the present time:

(1) A recent investigation has shown that the total value of all meridian observations made in the one-fourth of the sky nearest the

* These views are in the accompanying sub-appendix on a Solar Observatory, page 104.

southern pole is only one-fifth that which pertains to the one-fourth of the sky nearest the northern pole. For the faint stars the disparity is much greater. The observations for the determination of the positions of all stars down to the ninth magnitude have been practically completed down to the thirty-second parallel of south declination. It is not known that there is any immediate prospect of a further extension southward of these much-needed observations upon the programme of the German Astronomical Society, and it is difficult to see how any considerable part of the energies of existing southern observatories can be spared for this purpose. It will even be difficult for them to make the necessary meridian observations to provide proper deductions from the catalogue plates of the astrophotographic survey.

(2) The modern revival of interest in double-star measures has extended but feebly to the southern sky. There are no very large telescopes in the southern observatories. By far the largest is the eighteen-inch visual telescope paired with the photographic telescope of the McClean Equatorial at the Cape. Even this is not available for very much work upon double stars.

(3) Much work is being accomplished by several astronomers in the British colonies and at the Arequipa station of the Harvard Observatory upon variable stars. Much work is required, especially in that class of observations requiring telescopes larger than eight inches of aperture.

(4) There is increasing activity in the measurement of stellar parallaxes at northern observatories. Nothing in this line is now attempted upon the southern stars, and very little has ever been done there, except that under the direction of Gill with the Cape heliometer.

(5) The first measurements of the motions of southern stars in the line of sight are yet to be made by the Mills Expedition, which is soon to be sent to the southern hemisphere by the Lick Observatory; but even then we shall have two telescopes there pitted against six in the northern hemisphere. It is very important that another large reflector should be placed in the southern hemisphere and employed for several years, at least, in the measurement of motions in the line of sight.

(6) It has recently been pointed out that the proper investigation of the variation of latitude requires that there should be one or more observing stations in the southern hemisphere. This is a very real and immediate need.

(7) A systematic photographic survey of the nebulæ with a large reflecting telescope is needed at the present time Photographs should be taken of all important nebulæ in the northern and southern heavens, for comparison with similar photographs to be taken in the future.

(8) Investigations should be made with moderate and high dispersions on the spectra of southern stars in connection with the problem of stellar evolution. A large reflecting telescope would be needed for this work.

In relation to the lines of investigation enumerated in the fore going. it may be said that all have interest in and for themselves; but in relation to (1), (4), (5), (6), and (8), it may also be asserted that they have a further and very great interest, because the full value of work already accomplished at northern observatories in those lines will not be realized until the corresponding southern observations have been made.

In addition to the lines of investigation we have specified, there are many others which would be of a high degree of interest in relation to a southern observatory. In fact, there are very few activities in northern observatories—saving only experimental researches for the sake of opening new lines of investigation—which do not need to find their counterpart in southern observatories to an extent very much greater than is now possible for existing observatories in that quarter of the world

Site.—Such an observatory should be south of the thirtieth parallel of south latitude, if possible, for the following reasons:

(*a*) Because the elevation of the southern pole in all the researches specified in the first part of this communication ought to be at least thirty degrees above the horizon in order to secure the necessary steadiness of atmosphere in measurement upon objects near that pole.

(*b*) Because the proper combination of meridian observations with those of the northern hemisphere is best effected when the latitude of the southern observatory is as nearly as possible that of the northern.

(*c*) Meridian observations in right ascension as well as in declination require that the "seeing" shall be good for at least ten degrees below the pole.

(*d*) For an observatory which is to require prolonged and vigorous work of measurement, extreme tropical conditions, such as might seriously affect the health of observers should be avoided.

It might not be possible to accomplish all the objects desired by a single station, but in such case it would still be desirable to have one observatory as headquarters, and other stations in the most convenient possible relations with it.

In selecting a site, clear skies, dry and equable climate, and a fair degree of elevation above the sea level would obviously be desirable. yet accessibility, stability of local government, and cost of living might modify these considerations to some extent. It would be an advantage, perhaps, to have a site where two intervisible stations, of which one would be at a high altitude, could be maintained. This would be possible in Chile, where the climate is very favorable down to about 35° of south latitude, but where the danger to the fine adjustment of large instruments from earthquakes might be considerable. There would be some advantage in the selection of a point in the interior near Sydney, Australia, where the sky is sufficiently clear, the atmosphere dry, and the climate fairly suited to European and American constitutions. If solar observations should be undertaken at the proposed observatory, there would be a unique and obvious advantage for these, as for many other classes of observations, in having an observatory in a longitude differing so much from that of any existing observatory of importance. Many observations of planetary and other phenomena could be made when it is daytime for all the principal northern observatories. One distinct disadvantage of any station in southeastern Australia is found in the hot waves which prevail in the early months of the year.

South Africa offers a favorable site for the proposed observatory. The civil conditions there may be regarded as somewhat unsettled. The chief observatory of the southern hemisphere is already located there, and it might be regarded as somewhat undesirable that another strong observatory should be located near it.

Organization.—The kind of observatory which we would recommend to be established by the Carnegie Institution must necessarily depend upon the amount which would be available for its establishment and support. In our ignorance upon this point we are unable to present a definite plan, and we simply venture to offer a few suggestions.

Sums varying from $50,000 to $500,000 for plant could be judiciously expended for this observatory. Excellent results might be obtained with an annual maintenance of $20,000; while a much larger amount could be economically expended annually without the least fear that the share of this observatory would be overdone.

In the organization of the proposed observatory the matter of permanent staff would seem to be a secondary consideration. Whatever permanent staff might be maintained should be regarded as auxiliary to the main purpose—special researches. This staff should be held as disposable, on occasion, for the assistance of special investigators to whatever extent the necessity of the case might require. The installation of piers, observing rooms, and even of instruments, could be provided in advance for the use of special investigators by the small permanent staff.

A certain amount of regular observations could be assigned to the permanent staff, such as observations of double stars, variable stars, comets and small planets, when far south of the equator, daily photographs of the Sun, and other observations. For a long time to come it would probably be found desirable to maintain regular meridian observations of southern stars. One astronomer in charge, one or two astronomers of the grade of assistant, and two or three computers would probably suffice for the permanent staff at first.

The permanent equipment at first might consist of a visual telescope of from 12 to 18 inches aperture, a photographic doublet of from 10 to 12 inches aperture, a meridian circle, a zenith telescope, small telescopes, clocks, chronographs, etc.

The equipment for special researches ought to be provided in connection with the special occasion for it, and might, in some instances, remain as part of the permanent equipment at the expiration of the initial investigations which may have called it into existence.

The buildings should be of the most simple and inexpensive construction. The observing rooms should be built as needed, and should be of the lightest construction consistent with the protection and safety of the instruments they shelter.

There should be a simple office building, so planned as to admit of future extension to good advantage. It would probably be a good plan to provide inexpensive living quarters or barracks for observers.

It is to be hoped that the success of the observatory in inviting support would be such that a succession of special investigators, with their respective assistants, could be kept on the ground all the time; so that eventually the distinction between permanent and temporary staff would virtually disappear.

The end to be kept steadily in view should be not to devise employment for a large permanent staff, but to find men of special qualifications to be sent to the observatory from time to time to

accomplish work urgently needed for the progress of astronomy, for which provision can not equally well be made elsewhere.

This subject is so important and presents so many points of novelty that it would be impossible for your Committee during its brief remaining tenure of office to give it consideration adequate for the presentation even of a preliminary working plan. We have contented ourselves, therefore, with the foregoing general suggestions, and we would urge upon the Trustees of the Carnegie Institution the importance of a full and careful investigation by means of a commission specially appointed for that purpose.

EDWARD C. PICKERING, *Chairman*,
S. P. LANGLEY,
SIMON NEWCOMB,
GEORGE E HALE,
LEWIS BOSS,
Committee.

OCTOBER 21, 1902.

SUB-APPENDIX OF APPENDIX A TO REPORT OF COMMITTEE ON ASTRONOMY.

PROPOSAL FOR A DISTINCTLY SOLAR OBSERVATORY.

OCTOBER 20, 1902.

Professor GEORGE E. HALE:

I have been asked by you for information in some detail regarding the scheme of observations, installation of buildings, and cost of equipment of a solar subtropical observatory for studying the Solar Constant and allied problems at a great altitude, which was proposed in answer to a letter from Mr. Walcott.

I have just returned from Europe and am unprepared to give any exact estimate, but will in the little time at my disposal indicate in general terms what is needed. I have for this purpose taken a list of apparatus actually in use at the Astrophysical Observatory and extended by Mr. Charles G. Abbot, the aid acting in charge, to what might be desired in the proposed installation, as a basis for my estimate of cost.

In my mind the essential idea for the solar observatory is to have two adjacent solar installations in immediate sight of each other; one at a certain considerable altitude, the other at the very highest altitude at which an observer can work, perhaps even only with

special provisions for breathing, for the indispensable condition in future successful study of the heat radiation of the Sun is an altitude where the greater portion of our atmosphere lies below us. This is for the upper site only, which is in connection with the near lower observatory in view from the upper, where the routine work will be carried on.

I wish, while repeating that this altitude is indispensable for some of the objects of the proposed solar observatory, to observe that it is not so for all. I wish also to recall that the whole plan involves primarily the use of special apparatus for solar observations. This primary use admits, nevertheless, that much of this apparatus can be advantageously employed for the study of such other objects as the photography of nebulæ, the Moon, or visual or photographic topography of Mars, though you will notice that these latter purposes may demand special apparatus other than that here indicated, which concerns the Sun primarily.

A necessary preliminary for the choice of any such twin site will be an examination by an expert of the conditions of "seeing" in the daytime for solar purposes only, to find whether the seeing is good for *this* purpose, quite irrespective of the condition of the vision at night, and this should be done soon, if at all, since the time of the next sun-spot maximum draws near.

The researches of which I particularly treat here chiefly involve measures of the *heating* effects of radiation.

1. Principal Objects of Inquiry of a Distinctly Solar Observatory.

(a) To determine the "solar constant," so called—that is, the heat equivalent of the solar rays falling perpendicularly upon a given area outside of the earth's atmosphere in a second of time.

(b) Whether this quantity be fixed or variable, and if the latter, how it varies through a term of years, and especially what connection exists between such variation and the sun-spot cycle.

(c) To determine what absorption the solar beam experiences in passing through the earth's atmosphere. The complete answer to this question implies a knowledge of the transparency of all layers of the air—high, medium, and low—and for all wave-lengths. It should also imply a repetition at both stations of the detailed infra-red line spectrum research already made.

(d) What absorption does the solar beam experience in passing

through what may be termed the Sun's atmosphere, and is this absorption constant, or is it, as has been suggested, variable and the cause of the supposed variability of the "solar constant"?

(*e*) What differences are there in the radiation of different portions of the Sun's disc, such as spots, faculæ, and prominences, and what evidence may be afforded from this as to the nature of those phenomena?

(*f*) Another large class of inquiry is not directly connected with heat radiation, but involves the use of the spectroscope, photography, and probably electrical apparatus.

2. Plan of Observations.

Nature of the Work.—Nearly all these researches require a study of the intensity of radiation of selected solar rays of all wave-lengths. The most suitable method for this involves several steps. The first is to obtain by the aid of one or more plane reflectors and appropriate mechanism a fixed horizontal beam. Second, a large solar image must be formed, preferably by a single concave reflector of great focal length. Third, the desired region of the solar disc is selected from this image by receiving it upon a screen with a small aperture, which serves also as the slit of a spectroscopic train. Fourth, a large fixed arm-prism spectroscope, with concave reflectors for collimator and objective, forms the spectrum of the selected solar beam. Fifth, this spectrum falls upon a highly sensitive temperature-measuring instrument and is caused to march uniformly over the sensitive surface, wave-length after wave-length, at a rate fixed by an accurate clock. Sixth, the indications of this heat measurer, exhibiting themselves as to-and-fro rotations of a suspended mirror, are caused automatically to record a curve upon the photographic plate, itself driven at a uniform rate before the mirror by clock-work.

Applications of such Spectrum Energy Work to Particular Objects.—I do not purpose here to give a minute scheme of observations or a detailed statement of the apparatus and accessories which they will require, but merely to indicate the main features. A more minute description, both of procedure and of apparatus, has been drawn up and will be submitted later if desired.

The Atmospheric Absorption.—It will be apparent that question (*c*) (of page 105), relating to the amount and variability of terrestrial atmospheric absorption, must be solved before or at the same time

with the questions (*a*) and (*b*), relating to the amount and supposed variability of the "solar constant," for it is impossible to know how much of this total is lost before the measuring instrument is reached. Experiments near sea-level can never determine this correction with entire certainty, though made with the widely differing thicknesses of intervening air corresponding to different altitudes of the sun, for the atmosphere is almost unbelievably variable in its absorption from day to day and month to month, and even in different parts of an apparently clear day. Especially is this the case with its lower layers, and, besides this, no two layers of air at different altitudes are alike in their absorption. What is required is to get high up in clear air, so that the absorption will be much smaller (it amounts at sea-level to something like 50 per cent of the total radiation), and where that which is left is more constant in amount. Furthermore, dependence ought not to be placed exclusively on observations taken at the same elevation above sea-level, for, while the higher layers of air are less variable in absorption than the lower, and while the absolute amount of absorption is less the higher the observer, yet it is necessary also to take into account the variation in quality of the absorption with the elevation. A second lower station ought, therefore, to be occupied and simultaneous observations made at the high and the low station. This ought not to lead to dispensing with a lower observing station equipped equally as well for work as the higher, and where, indeed, it might prove feasible, *after the study of the air had progressed satisfactorily*, to do much of the observation on the "solar constant." It is clear that the extreme difficulty of observation upon the high station ought thus to be avoided just as far as possible.

Methods of Determining Absorption of the Air and the Value of the Solar Constant.—These problems are naturally to be studied simultaneously. In this general statement I will not describe the procedure special to each, but only indicate several kinds of data that ought to be used. First, solar energy curves taken on the same day at the high station through different air masses to be computed from the altitude of the Sun and the height of the barometer at the several times of observation; second, solar energy curves taken at the low station through similar differences of air mass; third, similar curves taken with the solar beam reflected on an approximate level between distant mirrors both at high and at low stations; fourth, actinometer observations coincident with these several kinds of bolographic work;

fifth, bolometric observations on the spectral energy distribution of the standard radiator or "black body."

The Absorption of the Sun's Atmosphere.—While there is not as good an opportunity to study the Sun's absorption as the Earth's, much may be learned by forming spectrum energy curves at various parts of the disc, for it is apparent that as the limb is approached greater and greater thicknesses of the outer solar layers must be traversed by the radiations from the interior. Thus a comparison of the ordinates of the spectrum energy curves of a solar beam starting at 98 per cent of a radius from the center with that of the central beam itself discloses a powerful absorption of the shorter wave-lengths in this lengthened path through the solar envelope amounting even in the yellow to more than 50 per cent of the whole radiation at the center. It has been supposed by some that there is considerable variation in the temperature and consequent absorption capacity of the solar envelope, and it is to this that Halm, in a recent paper, attributes the eleven-year sun-spot period. The investigation of this question through a term of years by the aid of spectrum energy curves from various parts of the Sun's disc would be of great interest. A comparison of such observations at the high and low station would prove whether or not such results, uninfluenced by the absorption of the Earth's atmosphere itself, may in future be obtained at low stations.

It is unnecessary, after what has been said, to describe at length the particular application of the general method to a study of sunspots, faculæ, and other solar features, as the suitability of it for this study is apparent.

3. Apparatus and Accessories Required.

The Coelostat.—The first essential for each station is the provision of a fixed horizontal beam of sunlight. While there is opportunity for difference of opinion as to the best provision for this purpose, I am disposed to recommend, on the score of simplicity and satisfactoriness of operation, the coelostat, so called, in preference to any siderostat. I would propose for each station a long polar axis driven at the rate of one revolution in 48 hours, by powerful clockwork, and capable of carrying in the plane of its center several plane mirrors. One of these mirrors, of not less than one meter aperture, is to be used for bolometric purposes. The others may be used to furnish fixed beams for other researches at the same time.

In order to avoid the shifting of the beam with varying declination of the Sun, each beam may encounter near the coelostat a second mirror itself capable of traveling north and south on a track, and to be moved every few days north or south as occasion requires. In order to use the principal coelostat mirror at favorable angles both forenoon and afternoon, its second mirror has two such tracks close to the coelostat, which connect by a curve around the instrument, so that the second mirror may be wheeled to the east or west track, according to the hour angle of the Sun. The other beams for other researches should have only one track position for their second mirrors, choosing the position most favorable for morning or for afternoon observation, as experience would dictate.

While the beams from the second mirrors could be reflected in any direction, a north-and-south one is to be chosen to insure entire invariability of position.

Accordingly the concave mirror of 30 inches or more aperture and of 200 feet focus would be located north or south* of the coelostat some 60 feet, and would send its beam horizontally through a tube *under* the coelostat, where the solar image is received at the slit of the spectro-bolometer apparatus.

I have prepared a further list of apparatus for solar work, with the prices of each piece, but in view of the brief time at my disposal to prepare this preliminary statement, I think it better to give here only the main points.

All this is for the study of solar radiation, principally but not exclusively for that of heat, and it is distinguished by the abundant use of large mirrors, plane and concave, usually in connection with the coelostat and catoptric telescopes. These are associated with special buildings maintaining the bolometric and galvanometric trains at constant temperature, and at the upper station there may be a special construction to enable the observer to work in a special atmosphere. There will also be a special teleferage system or other provision for ready transport to the upper station. There will be two dioptric telescopes of at least 12 inches aperture for photography, and adjuncts too numerous to be mentioned here, but which are indicated fully in a separate note, to be communicated later.

This list of apparatus can later, if desired, be furnished in detail, with estimates for cost of each piece.

The total estimated expenditure for this apparatus, with the buildings, is a little under $150,000; but this does not include the roads

* According to which hemisphere is selected for the observatory.

and other items, whose cost is so uncertain that I prefer to estimate the total expenditure at $200,000 for all such objects.

There has been prepared a scheme of expense for maintenance, including salaries of a director, at $5,000; an assistant director, at $3,000; two assistants, at $2,000 each, with mechanicians and others specifically named, which, with subsistence (necessarily to be provided by the observatory), materials, etc., aggregates $35,000 a year.

It is estimated that $300,000 and interest will provide for this for eleven years, and that the total expense of the distinctly solar observatory for the eleven-year period will be covered by the already named sum of $500,000.

The provision of a great southern subtropical observatory, if associated with this, will doubtless involve a far larger expenditure, which is not here immediately considered.

What here is immediately considered is the idea of a distinctly solar observatory, and even this not solely for its scientific interest, but for its immense possible utilities to the whole human race.

I may say, in illustration, that I am personally deeply interested in the study of nebulæ. I can not but see, however, the enormous difference in quality between this study and that of the Sun, for all the nebulæ in the sky might be blotted out without affecting the price of a laborer's dinner or the material comfort of a single human being. What shall we say of a similar contingency to the Sun? While a slight variation in the radiation of the Sun may conceivably cause the death of millions of men by famine, it certainly seems worth while to look at it from its utilitarian as well as from its purely scientific interest.

It is the possible immense *utility* of the solar observatory that I dwell upon, and concerning which I may borrow the weighty words of Professor Newcomb in a similar connection, and state that astronomical research in this direction may bring to light not merely interesting cosmical processes but "cosmical processes pregnant with the destiny of our race."

S. P. LANGLEY.

[*S. P. Langley to Mr. Walcott, February 28, 1902.*]

FEBRUARY 28, 1902.

DEAR MR. WALCOTT:

You were saying to me that you knew of some persons who might be desirous of aiding, through the Smithsonian Institution, some

large object, and I was led to write you what is in substance the following letter:

I learn from yours of February 14 that you would like to call it to the attention of the Executive Committee of the Carnegie Institution, and, as I have written, I shall be very glad to have you do so, asking you to make it clear that it is in no way a request from the Smithsonian Institution, but a suggestion from me of a great object which Mr. Carnegie himself may care to take up.

I do so the more readily because, considering the Institution wholly apart from its own needs, it would be the glad means of indicating to those who wish some worthy aim for expenditure, some specific object. which may be undertaken if desired *in their own name* and through any worthy medium they prefer.

One of these is the determination of the heat the Sun sends the Earth and the causes of its probable variation. The progress of solar physics has been such in the last few years as to make it of interest to every inhabitant of the planet that this progress should be carried further, not only in scientific, but in economic, and in even humanitarian interests.

The establishment of a great observatory in the tropical or subtropical regions at a high altitude would advance our knowledge of the heavenly bodies in a degree more than could be done by all the physical observatories in the world united. To the founder of such an observatory there would be enduring fame, but it is an affair of a very great deal of money, possibly to be reckoned only in millions. The establishment and maintenance for eleven years of a distinctly solar observatory under these conditions would enable us to study the sun as it has never yet been studied, and through an entire solar cycle, for much less cost.

While this latter research, then, is to be pursued at less cost than the foundation of a great general observatory, it has a specific object of literally world-wide importance and interest.

The determination of the heat the Sun sends the earth annually is the determination of that through which everything on the planet lives and moves, and almost unknown slight variations of this heat are the probable, if remote, cause of the changing character of the seasons and of the lack or plenty in the crops upon the Earth as a whole.

It has seemed possible within the last few years that if we had this knowledge, the years of plenty and of famine could be forecasted as we now forecast a coming storm through the advices of the

Weather Bureau. It is possible, I say, but I do not wish to say more than that it is possible.

I do not know any greater or more worthy object for the expenditure of $500,000 than the settlement of this latter great question would be. It is, with our present knowledge, almost a question of money; but no government is prepared to spend such a sum except for its own interest. This is for the interest of all the people in the whole world, and I entirely concur with the recommendation of its importance from the Chief of the United States Weather Bureau, which I enclose. I should gladly see it undertaken, whoever does it

Very truly yours,

S. P. LANGLEY,
Secretary.

The Honorable CHARLES D. WALCOTT.

APPENDIX B TO REPORT OF COMMITTEE ON ASTRONOMY.

PROGRESS AND PRESENT STATE OF ASTRONOMY.

BY LEWIS BOSS.

Professor E. C. PICKERING,
Chairman of the Advisory Committee on Astronomy,
The Carnegie Institution.

SIR: Acting upon the suggestion of the Advisory Committee on Astronomy that its individual members submit their views upon various specialties in astronomical research which could not be treated in detail in the general report, I beg leave to submit some memoranda which are suggested by my own experience in watching the progress of astronomy.

As a preface to this, I desire to suggest some views which I regard as of importance in determining the general policy of the Institution toward the support of astronomy.

The United States already has a large number of observatories for which the provision as to maintenance is notoriously small in relation to instrumental plant. There appears, therefore, to be little necessity for the establishment of new observatories or institutions, provided some means can be devised to make better use of those we have.

To construct a satisfactory working policy for the Carnegie Institution in its relation to astronomy, as well as to other sciences, is manifestly to be the work of time and experience. In a general way it might appear reasonable that the Institution should endeavor to accomplish distinct results in definite lines rather than to spread itself over the entire range of astronomy in a miscellaneous way without definite aims. Yet it might be difficult for the Institution to decide upon the directions in which it could most advantageously throw the weight of its support until experience shall have contributed to a solution of the problem. Among the objects it may decide to support at first will be found some that are worthy of continued recognition, both because of their great and obvious importance to the progress of astronomy and because of the efficiency of the particular investigators selected to carry them out.

The massing of miscellaneous astronomical investigations under a single executive head in a great institution does not commend itself to my judgment as economical or as likely to bring to the front the kind of power which is necessary for the highest form of research. In such an institution the main current is apt to be sluggish. It is true that such institutions are valuable when they can be controlled, on special occasions, to the exclusive support of some great investigation like those upon the planetary system which were carried on in different countries at different times by Le Verrier and Newcomb, and that they may also be valuable for purposes of stellar and planetary observation as illustrated at Greenwich, Cape of Good Hope, Paris, and Pulkowa. In general, their usefulness is not in proportion to the expenditure.

Furthermore, we have the expressed wishes of Mr. Carnegie in favor of arriving at the promotion of pure scientific investigation, so far as possible, through aid extended to existing institutions.

Astronomy might be efficiently aided through the maintenance of a new observatory in the southern hemisphere. It is not desirable that this should be an elaborate affair—merely an astronomical station, with a very small permanent staff to serve as a convenience or nucleus for expeditions sent out from the United States for special objects from time to time. Such expeditions have been sent from this country already. In 1850 Captain Gillis, of the Navy, was sent to Santiago de Chile to make observations of the southern stars. The Lick Observatory will shortly send an expedition to measure the motions in the line of sight of southern stars. Dr. Gould's great undertaking for observation of the southern stars, 1871–1884,

was in the nature of an expedition, though it was supported by and became the national observatory of the Argentine Republic. There is now very pressing necessity for such expeditions, as I shall endeavor to show further on.

The foregoing considerations seem to lead to the following suggestions to be commended to the consideration of the trustees as suitable to the present situation so far as it relates to the support of astronomy:

First. Assistance should be extended to institutions which already have in hand important researches in astronomy—institutions which have already demonstrated their usefulness and which have sufficient vitality for a good foundation, but not sufficient income to carry out their existing plans in a desirable way.

Second. To assist investigators of acknowledged ability, wherever found, to initiate or carry out works of high importance to the progress of astronomy; and in doing this to aim at giving the investigator a free hand.

Third. To take into consideration the establishment of an astronomical station in the southern hemisphere for the use of expeditionary forces to be sent out from the United States for the purpose of reducing the great disparity which now exists between observation upon objects in the southern sky, as compared with those in the northern.

As to the investigations in astronomy which are pressing for attention, they will be found to be exceedingly numerous. A few of these I shall attempt to suggest in the sketch which follows, leaving the fields of gravitational astronomy and astrophysics to be covered by other members of the Committee.

Planetary Observation.

The observation for position of the major planets, Sun, and Moon has always been the peculiar care of the Royal Observatory at Greenwich. Such observations have also been made at Paris, Washington, the Cape of Good Hope, and at other national observatories. It is as necessary as ever that these observations should be continued; and the national observatories can be depended upon to meet the demands of science in this respect. As to the observation of minor planets and comets, there is a lack of organized effort. Several observatories in Germany, France, and Italy devote a large share of their attention to these objects. At the Lick Observatory

the discovery and accurate observation of comets has been prosecuted for a long time with much judgment, ability, and success.

Some years ago substantial prizes were offered for the discovery of comets. In those days several enthusiastic amateurs were enlisted in this work, with the result that the sky was constantly patrolled, and astronomers had a reasonable assurance that no comet of any consequence was likely to escape discovery. This is a matter of importance, because it would be interesting to learn whether, as has been suspected, more comets are seen at the times of greatest solar activity, with the consequent inference that the envelopes of these comets are excited by the magnetic condition of the Sun.

Meteors.

The interest in the observation of meteors suffers no diminution. Of late years a systematic effort has been made to photograph some of these objects in flight. These efforts have been attended with partial success. By means of photography, an incalculable advance in the exact astronomy of these bodies becomes possible, and this method is deserving of encouragement. This work has been carried on for several years at the Winchester Observatory of Yale University.

Double Stars.

Thirty years ago interest in the observation of double stars appeared to be on the wane. The unsatisfactory nature of the orbital computations which were made from time to time on the basis of exceedingly minute quantities of slowly changing relations led to something like a feeling of despair. Comparatively few binary stars have a period of revolution less than one hundred years. Of the vast majority the period of revolution is at present entirely conjectural. Revival of interest in this branch of astronomy was largely due to an American amateur astronomer, Mr. S. W. Burnham, who began his labors with a small telescope. Graduating from this into regular astronomical employment, with the use of large telescopes Mr. Burnham added a large number of discoveries of new and interesting double stars to the list of those already known. He made a very large number of measurements upon these objects. New interest was manifested by other observatories. The large telescopes of the Lick Observatory, of the observatories of Cincinnati, Madison (Wis.), and Washington, of the Northwestern University, and of

the University of Pennsylvania and elsewhere, as well as at various observatories in Europe and the southern hemisphere, have been directed with good effect upon this work; so that there is no reason to complain of a deficiency in this class of observations. The part borne by America in this campaign is extremely creditable and in most refreshing contrast to the history of American astronomy in this respect previous to 1870. In comparison with other branches of astronomical work, the need for increased activity in this line is, perhaps, relatively less pressing.

Researches upon the System of the Stars.

Under this general head may be included a very important part of the work of astronomers of the present day. We may construe it as including meridian and other observations for the position and motion of stars; observations to determine astronomical constants, such as refraction, aberration, and nutation; observations for variation of latitude; determination of parallax, and a great variety of researches which require fundamental observations of precision as the basis. Included in this class of investigations also are meridian observations of planets, upon which our planetary theories are based, since such observations must be made in the same way and in the same series as those of high precision upon the stars.

Observations for Positions of Stars.

There are more than twenty-five observatories in the world wherein observations to determine the positions of stars form the principal activity. There are as many more observatories where such observations are an important part of the work undertaken. At the great national observatories located at Pulkowa, in Russia; Greenwich, in England; Paris, in France, and Cape of Good Hope, in South Africa, in some form or other, such observations are the chief employment, and the fame of those institutions rests chiefly upon what they have accomplished in observations of this class.

We have first exploratory and statistical observations where identification and enumeration are the principal objects to be attained. The great *Durchmusterung* carried out at the Bonn Observatory by Argelander and Schönfeld, the similar survey undertaken at the Cordova Observatory, in the Argentine Republic, and the photographic survey completed at the Cape of Good Hope are the leading

and most effective observations of this class. Nothing of great importance in this line of work has been accomplished in this country. In this same line should be included the great photographic survey now going on under the leadership of the observatory at Paris, which has for its declared object the delineation upon photographic charts of all stars down to the fourteenth magnitude in the entire heavens. There are some indications of lagging in this, the main feature of the coöperative plan described, owing to the great difficulties in execution and the very great estimated cost of reproduction of the plates upon printed charts. This country has taken no share in this great work; and this may be considered a matter of regret. Undertakings of this kind indicate far-seeing insight into the needs of the astronomy of the future, and a spirit of enterprise in the breaking of new paths of investigation. Even the mere counting of stars by Herschel (his "star-gauges") was the foundation of a new philosophy of the heavens--one of the most fruitful chapters in our science. How much greater things may be expected from the charting of the whole heavens upon a uniform plan may easily be imagined. This plan is most interesting from what we know it will accomplish, and not less interesting from the unknown possibilities which may flow from such works.

FUNDAMENTAL STAR POSITIONS.

At the other extreme of the scale is found the determination of the positions of the principal stars—fundamental or absolute observation, as it is called. This problem is twofold. First, we have to find an invariable line of reference, or direction, in space—philosophically one of the most interesting problems which has been attempted in the whole range of exact sciences. The earth is rotating on its axis, with the direction of its axis continually disturbed by a multitude of external attractions and terrestrial perturbations. At the same time, the earth revolves in its orbit about the Sun subject to a multitude of perturbations disturbing uniformity of motion. The earth has a third motion with the Sun, at a high velocity, through space in a direction now approximately known. The problem is to determine with humanly absolute exactness the direction of some line in space in such a manner that it can be identified with certainty at any future time.

The second problem of fundamental astronomy is to ascertain with the highest precision what may be termed the graduation of the sky.

In other words, we strive to determine with reference to some fixed system of imaginary circles upon the sky the location of the brighter stars. This results in the determination of the positions and motions of the stars—the practical outcome. These positions, so determined, become the basis of reference for planetary astronomy, for the tables of the nautical and astronomical almanacs, and for the extension of the work of precision by mere differential processes to the great multitude of fainter stars. Thus, the fundamental observation of the principal stars is the real foundation upon which rests the entire structure of the astronomy of position.

The leadership in extent and importance of work in this line must be accorded to the observatory at Pulkowa and to the two English Royal observatories, respectively at Greenwich and the Cape of Good Hope. Good work of this kind has been accomplished at various other observatories in Europe, but none in the United States up to the present time.

There is, however, a class of observations of precision falling little short of those which are really fundamental, in respect to which the instances of successful accomplishment are more numerous. Some of the notable contributors in this line have been the observatories at Dorpat, in Russia; Königsberg, Berlin, Bonn, and Strassburg, in Germany; Paris, in France; Leiden, in Holland; Oxford, Cambridge, and Edinburgh, in Great Britain; Melbourne, in Australia; Madras, in India; Cordova, in the Argentine Republic, and the observatories of Washington, Mount Hamilton, Cambridge, Cincinnati, Madison (Wis.), and Albany.

After these, upon a scale of precision scarcely inferior in many instances and on a par in importance, rank the observations of the positions of faint stars by secondary methods, either with the meridian circle or through astronomical photography. This is distinct from the general or exploratory charting, which has been described and which is the step naturally preliminary to measurement of precision. The historic zone observations by Lalande, Bessel, Lamont, Argelander, and Gould are examples of this class. To observations of this class we owe a large share of the exact knowledge which has hitherto been acquired as to what is happening in the stellar universe. Some thirty years ago the German Astronomical Society assumed leadership in an undertaking to observe all stars down to the ninth magnitude in the northern sky (over 100,000 stars). The Harvard College Observatory and the Dudley Observatory were participants in this work. The former

observatory and the observatory at Washington have taken part in the extension of this work southward. Lately, at Albany, observations of stars down to the eighth magnitude have been extended to the thirty-seventh parallel of south declination, the results of which are now in preparation to form a catalogue of 10,000 stars.

Included in the program of the great astrophotographic survey under the leadership of the National Observatory at Paris (to which allusion has already been made in the foregoing), is a plan to determine through measurement of photographic plates the positions of all stars brighter than the eleventh magnitude. This gigantic undertaking is proceeding with a success as to the northern hemisphere which scarcely could have been anticipated by men practically acquainted with the difficulty of such tasks. The state of the work in the southern hemisphere is far less flourishing, though the situation is not without an element of hope. The burden of this great enterprise has fallen very largely upon the English and French governments. The United States has taken no part in it.

It may become desirable in the future that the Carnegie Institution should undertake a section of the southern sky, should some one, or more, of the participants in the southern hemisphere fail—a contingency not without the bounds of possibility.

In the foregoing paragraphs I have indicated in a very summary way what has been accomplished in observations of precision upon the relative positions of stars. This work has exercised the energies of considerably more than one-half the working force of astronomers during the nineteenth century. The observatories already established for, and now engaged in, this work may be depended upon to keep up a fair balance of output in relation to other branches of astronomy, so far as the ordinary types of such work is concerned, and in relation to the northern hemisphere. In all countries except the United States, governments may be relied upon to render fairly adequate support to this class of observations, since the relations of this work to the practical uses of mankind, notably in geography and navigation, appeal more directly to the motives of official administration.

Fundamental determinations of star-positions, however, belong to a class of highly specialized researches, giving free play to originality of conception and invention of methods. Only a very few of those which have been made in the entire history of modern astronomy are entitled to the claim of high rank. To sustain the proper balance toward differential, or secondary, observations, there should

have been twice as many. This work is extremely laborious in extent of its details and exacting in the attention it requires to minute and hidden sources of discordance It has been described as the most severe example of physical investigation in the exact sciences. When the opportunity offers, such researches might appropriately be assisted by the Carnegie Institution to the great profit of astronomical progress.

In fact, there is no lack of novelty and scientific interest in meridian observations for positions of stars when they are directed toward the investigation of special problems which require observations in a special field. In view of the relations of our government to astronomy, it also seems proper that the Carnegie Institution should attempt more in this line than it might under different circumstances.

There is great deficiency of such observations in the southern hemisphere, while for the purposes of astronomy it is important that the southern hemisphere should receive equal attention with the northern. There is especial need of meridian observations in that hemisphere now. Sir David Gill, Astronomer Royal at the Cape of Good Hope, is accomplishing a great amount of meridian observation of a very high grade, and measures lately taken by him seem to insure still greater and better results in the future. But a single observatory is entirely unable to cope with the situation; and, although much good work may be expected from other observatories south of the equator, there is pressing need that further provision for meridian observations of all classes should be made. The location of an observatory for this purpose should be south of the thirtieth parallel. The Harvard Observatory has extended its photometric and photographic patrol to the south pole by means of its station at Arequipa, in Peru. Recently the Lick Observatory has dispatched an expedition to extend its determination of the motion of stars in the line of sight to the brighter stars of the southern skies. That fundamental and secondary meridian observations should be made in supplement to existing forces in that quarter of the world is sufficiently obvious to all who have closely studied the conditions of the problem involved. This could be accomplished under a plan resembling, with modifications, that which was adopted by Dr. Gould thirty years ago. The United States has no territory in latitudes suited to the establishment of such an observatory, and there is small likelihood that its attention would be favorably enlisted in the consideration of such a plan. Such an observatory might be a very simple affair. It might be purely expeditionary in form, and it

might even be supplied with instruments temporarily borrowed from some northern observatory.

In general, that class of investigations in this department of astronomy, which require some degree of departure from existing routine, are very likely to require attention at times through supplementary aid. The whole subject is one well worthy of the attention of the Carnegie Institution.

DEDUCTION FROM MERIDIAN OBSERVATIONS.

The observer in this department of astronomy corresponds very well with the skilled collector in the various branches of natural history. The observations, which have been accumulated for more than a century, must be collated and compared. They must be brought to bear upon the solution of physical problems They are dead material until they have been used in this way. Very little has been done with this vast mass of precision observations in the way of bringing its results to bear upon questions concerning the motions of the stars, the motion of the Sun through space, and the structure of the sidereal universe. The answers to a variety of most important questions in cosmogony that are propounded and vaguely discussed lie hidden in that mass of material, answers which very probably may bring us only a few steps forward, but which are certain to accomplish as much as that.

Comparatively little is yet known about the motions of the stars. Not one-tenth of the available observations has been brought to bear on that subject. Scarcely a greater percentage has been utilized in the discussion of the all-important constant of precession, and the same is true as to that problem of most absorbing interest—the solar motion.

Within the last three years the immense work of collecting and comparing this mass of meridian observations has been undertaken by Dr. Ristenpart, under the auspices of the Berlin Academy. Experience will show that this work must be continuous, since it will no sooner be completed to a certain date than the necessity will arise for extending to a later one. Yet it is from work like this that we must derive nearly the whole of the benefit to the progress of astronomy which is to be expected as the ultimate reward for the immense energy which has been expended in making meridian observations of precision.

For many years a similar collation and comparison has been in progress at the Dudley Observatory of Albany, restricted to stars

known to have sensible motions, or to be brighter than the seventh magnitude. This has been undertaken with the purpose of founding upon it a comprehensive examination of problems relating to precession, solar motion, and generally to systematic motions of the stars. In this way only is it possible to gain exact knowledge as to the mechanism of the heavens.

Reduction of Old Observations.

In recent years there has been great activity in the work of revising the computations relating to series of meridian observations made prior to the middle of the last century. This is a most important department of astronomical effort. Several years ago Dr. Auwers, of Berlin, completed a most painstaking and masterly revision of the star catalogue founded on Bradley's observations of mean date about 1755. He also effected a new reduction of Mayer's less important observations of about the same date. Stone and Gill have completed the reduction and publication of the long series of observations made at the Observatory of the Cape of Good Hope. Frisby and Brown, of the Naval Observatory, have reduced the zone observations of Gilliss at Santiago in 1850. Weiss has revised Argelander's southern zones of 1850, and Seeliger those of Lamont of an earlier date. Dr. Copeland, of Edinburgh, has undertaken to revise the reductions for Henderson's valuable series of meridian observations made at Edinburgh around 1840. Dr. Downing, of the British Nautical Almanac, has published a revision of Taylor's Madras observations contained in his star catalogue for 1835. For some years Dr. Hermann Davis, of the U. S. Coast and Geodetic Survey, has been engaged upon a new reduction of Piazzi's observations made at Palermo about 1800. Recently a new reduction of Groombridge's observations, 1810, has been undertaken at the Greenwich Observatory.[1] Dr. Auwers has recently published a most valuable star catalogue, compiled from the Greenwich observations of Pond about 1815, and Dr. Chandler long ago proved the very great value which would attach to a new reduction of Pond's later (and best) work about 1830.

It is extremely desirable that this latter work should be taken up and pushed to completion, since it would undoubtedly result in a star catalogue of very high precision, fairly comparable in accuracy with the very best observations of Struve and Argelander and superior to them in some respects.

This field is important, and much remains to be done. One is tempted to urge that an organized effort be made to revise all the older star catalogues and planetary observations in a manner to render their best results available for the immediate and pressing needs of astronomical investigation. Certainly this work is worthy of aid wherever it is undertaken upon the initiative of competent hands.

Determination of Stellar Parallax.

Closely associated with the work of determining the positions and motions of the stars is the problem of determining their distances through measurement of parallax. Sixty years ago Bessel was the first to measure successfully the parallax of a star, after this problem had baffled the most strenuous efforts of many able astronomers from time to time in the past. Since Bessel's measure of the parallax of 61 Cygni other measures have been slowly accumulating, until, within recent years, there has been very greatly increased activity in this line of researches. Parallaxes are measured by many different methods, of which those most approved at present are by means of the heliometer, by means of photography, and by differential transits on the meridian. Gill at the Cape of Good Hope and Elkin at New Haven have determined a large number of parallaxes by measurements with a heliometer. Recently Elkin and Chase have been carrying on a kind of parallactic survey upon a very large number of stars. The photographic method has been tried with some degree of success by Pritchard of Oxford, and by others. The transit method was first illustrated by Kapteyn, at Leiden, and more recently by Flint at Madison (Wis.). Investigators are beginning to realize that the measurement, one by one, of the parallaxes of individual stars is a task which can no longer be postponed. There is every reason to believe that the photographic method, when applied with the aid of a large telescope of special design, like that proposed at the Yerkes Observatory, may yield results economically and, in all probability, of a high degree of accuracy.

Variation of Latitude.

Researches upon this subject have been of great interest ever since Dr. Chandler discovered, a few years ago, that the pole of the earth's figure rotates about the pole of rotation. The consequences of this phenomenon are so interwoven with the determination of the

places of the heavenly bodies and of the great constants of astronomy as to induce an organized effort to investigate its character in the most complete and precise way. A large number of series of observations has been made during the last seven or eight years with the special object of determining the actual variations of latitude in various quarters of the world Recently, upon the initiative of the German Geodetic Association, international observing stations, suitably distributed upon the same parallel of latitude, have been occupied by astronomers. Such a station has been established at Gaithersburg, Md., and at Ukiah, California: and another is provided by the Observatory at Cincinnati. It has been strongly urged that similar stations should be established in the southern hemisphere. This is a subject which is well worthy the attention of our government, and should be urgently pressed for its favorable consideration. It is quite possible that in the future this subject in some of its phases may deserve serious consideration by the Carnegie Institution.

Astronomical Constants.

None of the great constants of astronomy is yet determined with the accuracy which modern science demands. In recent years the solar parallax has demanded an amount of attention which seemed at times disproportionate to that which was devoted to other important fields of astronomical effort. The support which was extended by governments to expeditions for the observation of transits of Venus in 1874 and 1882 was munificent beyond precedent, and probably beyond the actual requirements of the case. Our government was even more liberal than any other. It is not known whether the observations by parties sent out from the United States were ever completely reduced. Twenty years after the last transit find them still unpublished.

Meanwhile other methods for the determination of the solar parallax have been applied, apparently with a result worthy of greater confidence than that due to observations of transits. Nearly a quarter of a century ago Gill employed the heliometer with good result in observations upon Mars at Ascension Island. Later, Gill and others employed the heliometer in coöperating observations upon minor planets favorably situated for the determination of parallax. The constant which is generally adopted at present virtually rests upon these observations. A few years ago a new planet, Eros, was discovered, remarkable for its eccentricity and its occasional

near approach to the earth. In 1900 occurred an opposition of this planet which was more than ordinarily favorable for the determination of parallax. A very elaborate scheme of observations by observatories in coöperation was organized under the auspices of the Paris Observatory. An immense mass of observations was accumulated, especially of photographs. There seems to be a suspicion that the computers are somewhat paralyzed by the very abundance of material which exists in these photographic plates contributed from all parts of the world. At the Lick Observatory, as well as at some others in this country, a very large number of plates was secured and is now awaiting measurement and reduction. It appears that little or no progress in this work has been effected as yet, and doubtless the Carnegie Institution may be asked to consider how far and upon what principles it is willing to extend its aid in carrying on this work. One is tempted to observe that zeal in taking photographs and reluctance in making measurements upon them when taken is rather characteristic of this branch of astronomy when applied to special objects. The office of the photograph is to bring down the counterfeit presentment of the sky to the laboratory desk, where measurements can be made at ease. The photograph does not eliminate the necessity for real work. It is simply supposed to make the work of measurement easier. The real contribution to science does not fairly begin until the photographs have been measured, and it does not seem necessarily to follow that because a stack of photographs has been taken with a certain object in view, these must be measured in order to save the results to science.

Aberration of Light.

In recent years interest in the constant of aberration was renewed in a remarkable degree. Astronomers, after resting for nearly a half century apparently satisfied with Struve's historic determination, suddenly suspected that it might be subject to serious revision. For several years there has been very great activity in observations and computations bearing upon this constant. The determination of this constant is interesting, both from its relations to the solar parallax and to the velocity of light. It is extremely desirable, therefore, that this constant should receive the attention astronomers now seem disposed to accord to it, and that variety of method should be encouraged. Recently in this country determinations have been made by Professor Comstock, of Madison, employing the

Loewy method; by Professor Rees at Columbia, and by Professor Doolittle at the University of Pennsylvania, using the zenith telescope, and by Professor Asaph Hall, Jr., at Ann Arbor, from meridian circle observations. The discordance of the results seems to render it desirable that further determinations should be made. Quite recently Dr. Chandler has carried on a series of critical investigations by computation upon all the series of observations available in the discussion of this constant, and he has arrived at conclusions of remarkable interest.

Constant of Precession.

The constant of precession is also of very great fundamental importance in its bearing upon the entire range of astronomy of precision as well as upon the testimony upon which we must ultimately rely for our investigation of the mechanism of the universe. This constant, however, stands in such intimate relation to the solar motion that the discussion of the two is practically inseparable. Special observations for determination of this constant are not necessary.

It should be mentioned that in Professor Newcomb's great work upon the planetary system he devoted much attention to the evaluation of a series of astronomical constants which should be consistent with each other and as precise as possible upon the basis of the material of discussion available to him. These constants have been generally adopted in the construction of astronomical almanacs; and it is characteristic of the rapid progress of astronomy that there has been no time within the memory of living astronomers when there has been apparently less satisfaction in the existing values of those constants which have been adopted. This is not because Professor Newcomb's results are not an improvement upon what had gone before, but because of the rapid accumulation of material of observation which points to the chance of deriving more reliable values, corresponding to the more exacting demands of astronomy.

Related Lines of Research.

From an analysis of the foregoing, it will be seen that there are several related lines of investigation pressing for attention, all bearing upon a distinct problem—that concerning the structure of the sidereal universe.

Of very high importance in discussing the minute motions of the stars is it that the observations of position should be freed from their

systematic errors—errors which, though they are not important in the determination of the motion of one star, are vital when masses of motions are treated to determine the general drift of stars. To arrive as closely as possible to freedom from systematic error, we must multiply fundamental determinations of position.

For the study of the mechanism of the sidereal universe we need to know the motions of the stars upon the basis of all the observations which have been made, correcting these, so far as possible, for their systematic errors. Then we shall be able to arrive at a better determination of the solar motion than has been thought of up to the present time, and we shall develop new ideas as to the drift of stars in various regions of sky.

This research, associated with determination of stellar parallax and with the measurement of motion in the line of sight, makes a strong combination for solving the problems that concern the distribution of the brighter stars in space which is relatively near to the solar system.

Determination of more accurate constants of aberration and precession are so interwoven with this problem that they must be considered an integral part of it. The same, in a modified degree, is true of the measurements of variation of latitude.

Without prejudice to other important lines of astronomical research, these investigations seem to offer a peculiarly favorable opportunity for concerted effort toward a highly important end to be pursued for several years to come. With the aid which the Carnegie Institution might be able to render, this program could be pushed forward with great certainty to a successful stage of development. This programme offers the advantage of a distinct purpose toward which many lines of research converge, each important in itself and all deriving greater importance from their intimate association.

Respectfully submitted.

LEWIS BOSS.

APPENDIX C TO REPORT OF COMMITTEE ON ASTRONOMY.

PROGRESS AND PRESENT STATE OF CERTAIN DEPARTMENTS OF ASTROPHYSICAL RESEARCH.

BY GEORGE E. HALE.

Professor E. C. PICKERING,
Chairman of Advisory Committee on Astronomy.

SIR. The remarks in this paper are made with special reference to certain researches which deal with the physical and chemical constitution of the heavenly bodies. The science of astrophysics, which makes such researches its principal object, has developed with great rapidity during the half century that has elapsed since its foundations were laid by Kirchhoff and Bunsen. From beginnings which of necessity involved much work of a purely qualitative character, the science has advanced to a position in which the demand for precision is no less exacting and is no less fully attained than in the most rigorous astronomical investigations. The nature of the methods employed and the possibilities of instrumental construction permit us to hope that a still higher degree of precision may be expected in the immediate future.

The general suggestions embodied in the report of the Advisory Committee meet my views at all points. There can be no doubt that the most serious need of funds is for the more complete utilization of existing equipment. One has only to contrast the extensive instrumental outfit of our observatories with the wholly inadequate provision for assistants and computers in order to realize this. Furthermore, there is great need for the establishment of an astronomical institution which can provide facilities, especially in the southern hemisphere, for the prosecution of special researches. The plan for this observatory need not be discussed here, as it is fully set forth in Appendix A. But I wish to offer some remarks on the importance of new instruments, as it is possible that the statement of the Committee on this subject may be misinterpreted by some who are unfamiliar with the nature of astrophysical research. Let me repeat, however, that I regard the needs here suggested as second in importance to the present requirement of more assistants and computers.

The extensive equipment of American observatories has been alluded to in the report. Without important additions of apparatus, twice or three times the present number of workers could find profitable employment for many years in making observations with existing instruments, and reducing them. But this fact by no means proves that our present equipment, powerful though it be, is not open to radical improvements, in which may lie the greatest hope of future progress. The history of science has shown that a single improvement in instruments may render years of work with inferior apparatus unnecessary. This is particularly true of astrophysical research, which, in view of the nature of the phenomena investigated and the possibility of employing the innumerable devices of the physicist, cannot be considered as subject to precisely the same conditions that obtain in researches dealing exclusively with the positions and motions of the heavenly bodies. In such researches the instruments and observational methods are comparatively few in number, and in the course of years a definite system of procedure, departed from only in matters of detail, has been gradually evolved. But astrophysics, which is so closely akin to physics, may be regarded almost in the aspect of an experimental science, susceptible of indefinite growth in method and in scope. No sharp line can be drawn separating its investigations from those of astronomy on the one hand or those of physics on the other. The primary interest of the astrophysicist may be an astronomical one; but it may equally well be that of the physicist, utilizing stellar temperatures and pressures to solve problems of atomic structure for which laboratory appliances are inadequate.

Under these circumstances it will be understood that as long as astrophysics continues to be a growing science the provision of new instruments for its researches will be necessary. A concrete example, drawn from an investigation of the evolution of the red stars, will illustrate the nature of the instrumental needs that constantly arise in the present state of the subject.

The study of stellar evolution is based upon the spectroscopic analysis of starlight. With our largest telescopes it is perfectly possible to photograph the spectra of stars much fainter than those at the limit of naked-eye vision. Measurements of the positions of the lines in these spectra determine the chemical composition of the star's atmosphere, set an upper limit to the pressure in it, and give an accurate value of the star's velocity in the direction of the earth. In most cases such spectra can be arranged in a definite series,

showing the gradual changes from star to star, which mark the steps in their evolution from nebulæ.

Every research bearing on stellar evolution is certain to raise questions of great interest, to answer which additional stellar, solar, or laboratory researches will be required. The spectra of red stars, for example, have been found to resemble in a most interesting way the spectra of Sun spots. On the strength of this one might be tempted to conclude that these stars, which probably represent the last stage of stellar evolution, are covered with spots like those on the Sun. Such a conclusion would have an important bearing on theories of the Sun, and perhaps on theories of variable stars. But with existing instruments the question cannot be settled. What is needed is a powerful grating spectroscope, such as Rowland used in his work on the Sun, mounted immovably in a constant temperature laboratory, and supplied with starlight by a special form of reflecting telescope. The special conditions thus realized would permit the exposure of a photograph to be continued from night to night, thus compensating for the faintness of the highly dispersed starlight. Even with very long exposures, however, only a few of the brightest red stars could be investigated in this way. For this reason the reflecting telescope employed should be of the greatest possible aperture. Many other astrophysical researches, such as the determination of the amount of heat radiated by the stars, could be made with such apparatus.

Thus one of the first questions raised by this study of stellar evolution suggests a new combination of instruments, which should render possible many important advances. Another question illustrates the urgent need for solar research. Available information regarding the spectra of Sun spots is wholly inadequate for the purposes of this investigation of the red stars, or for any systematic study of the spots themselves. It is remarkable that so many applications of the best spectroscopic methods to solar investigations are still to be made. Here, as in the case of the red stars, the nature of the work to be done is such as to require the use of instruments now employed only in the physical laboratory. If a small fraction of the enthusiasm so admirably shown on the occasion of total eclipses with a moderate share of the funds could be devoted to solar research at home, our knowledge of the Sun would lose nothing by the exchange.

But even the simultaneous study of the Sun and stars with instruments much better adapted for the purpose than those now employed

will not suffice in a thorough investigation of stellar evolution. Constant appeal must be made to laboratory experiments for the elucidation of celestial phenomena. The fact that some of the information required may be obtained at a future date in some physical laboratory by no means meets the need for immediate results. Cases are constantly arising in which much time could be saved if a good collection of physical apparatus, especially designed for radiation experiments, were at hand. But it is not only for incidental researches that such apparatus is needed. The advantage of carrying on extended physical investigations in direct connection with solar and stellar work is frequently very great. Evidence of this, based upon the peculiar radiation phenomena of hydrogen in certain stars and of carbon in others, the lack of extensive information regarding the effect of pressure on radiation, etc., might be multiplied indefinitely. The bearing of this on the importance of laboratory work in observatories and incidentally on the nature of astrophysics and its needs is sufficiently obvious.

This illustration of the intimate relationship of solar, stellar, and laboratory phenomena, drawn from a single research on the evolution of the red stars, is intended to give an idea of the variety of the questions which present themselves in astrophysical investigations and the necessity of providing new instruments to answer them. Consideration of such questions as these has led to the following conclusions, which seem likely to have some bearing on the progress of astrophysics.

1. In many investigations it is desirable that stellar, solar, and laboratory researches should be planned and executed from a single point of view, and, if possible, in a single institution. For this reason observatories in which astrophysical work is done should be well equipped with physical apparatus in order that special laboratory researches can be made when necessary.

2. Solar researches in great variety are urgently needed. Few fields of investigation offer such opportunities for important advances.

3. Laboratory conditions should be more completely realized in stellar and solar research. In this connection it is worth while to recall the fact that the spectroscope, even more truly than the telescope, is the chief instrument of the spectroscopist, and hence that provision should be made for its use under the most favorable conditions.

4. Much attention should be devoted to the improvement of photographic plates and processes. A comparatively small advance in this direction may double the power of a telescope.

5. The development of new instruments and methods should be encouraged. The more general provision of instrument shops in connection with observatories is to be recommended for this purpose

6. The construction of large reflecting telescopes offers the greatest possibility of improving the instrumental means of astrophysical research For all classes of stellar spectroscopic work, the measurement of the heat radiated by the stars, the photography of nebulæ, and many similar investigations, the reflector has very great advantages over the refractor. It is safe to predict that many future advances will be made with its aid. (See page 141.)

In closing these introductory remarks, I wish to add a word on the question of coöperation. In seeking to further the interests of astronomical and astrophysical research through coöperative effort, it seems to me that great care must be taken not to defeat what is perhaps the highest aim of the Carnegie Institution—the encouragement and development of individual genius. Cases will doubtless arise in which guidance of many workers by some well-known authority may be desirable and even necessary to the accomplishment of far-reaching results; but for the most part such cases will involve only the application of routine methods, sufficient for the collection of a mass of data needed in certain investigations. My argument is directed mainly against the centralization of authority and its concentration in a few individuals, however able they may be. It seems probable that the greatest progress will come rather through the advent of new minds, each competent to select its own point of view and to plan researches unhampered by the regulations of established systems. For this reason I believe that the best returns will be realized from assistance given to individual workers, coöperating, it may well be, with others, but constantly encouraged to advance science through the development of ideas and methods of their own.

The following sections contain statements of the progress and present state of certain departments of astrophysical research, for reference in connection with the report of the Advisory Committee.

Stellar Spectroscopy.

Although Fraunhofer discovered in 1823 that the spectra of stars differ among themselves and from the spectrum of the Sun, it was not until 1859 that spectroscopy was established upon a firm basis by Kirchhoff and Bunsen. Their analysis of sunlight by an instrument which permits the chemical and physical phenomena of distant

luminous objects to be studied with certainty served as a foundation for the recent remarkable development of this subject. Secchi, shortly after the chemical constitution of the Sun had been discovered, placed a prism over the objective of his telescope in the manner instituted by Fraunhofer, and proceeded to survey the spectra of the stars. In 1863 he published his first classification, which showed that white, yellow, and red stars have characteristic spectra, so related as to indicate a general line of development. A short time later Rutherfurd, who had initiated such work in the United States, published a similar classification. In 1864 Huggins entered upon his career as a pioneer in celestial spectroscopy. The record of his discoveries is too extensive for more than the barest reference here. He first directed his attention to stellar spectra, and after making visual observations which established the presence of iron and other elements in the stars, he attempted to record their spectra by photography. His first plates, though they contained impressions of the spectra, did not reveal the lines. These lines were first successfully photographed in 1872 by Dr. Henry Draper, who actively pursued this work until his death in 1882. In 1876, with more powerful instrumental means, Huggins returned to the task with great success. Thanks to the use of suitable refractive and dispersive media, his photographs reach far into the ultra-violet. They revealed at once the characteristic series of lines in the spectrum of hydrogen, previously unknown even in laboratory researches. The publication by Balmer of an empirical formula which accurately represents the lines of this series stimulated the studies of spectral series by Kayser and Runge, Rydberg, and others, which have so greatly increased the range and significance of spectroscopic research.

Two fields of investigation, both successfully cultivated in recent years, were opened up by Huggins. His identifications of spectral lines were significant, not merely in disclosing the chemical composition of stellar atmospheres, but also in suggesting relationships of star to star which could be possible only on the assumption of descent from a similar ancestor. The investigation of stellar evolution, foreshadowed in the classifications of Secchi and Rutherfurd, was thus placed upon a sure footing. Through the researches of Huggins, Vogel, Dunér, Pickering, Lockyer, and others, the line of development from gaseous nebulæ through Sirian and solar stars to the cooling red stars has been traced out with few elements of uncertainty. Where uncertainty still exists, it almost invariably arises from the fact that many of the most interesting and impor-

tant stars are very faint. The future study of stellar evolution, especially in its more detailed aspects, which have as yet received little attention, will, therefore, in large measure be confined to the most powerful telescopes obtainable. In work of this kind every inch of aperture counts, and instruments far larger than any now available could be used to immense advantage.

The other field of stellar spectroscopic research opened up by Huggins relates to the motions of the stars in the line of sight. Early recognizing the astronomical significance of Doppler's principle, Huggins proceeded in 1868 to determine stellar motions with its aid. Viewed from our present standpoint, the attempt was a bold one, for with the instruments at his disposal and in the absence of the almost indispensable aid of photography, the detection of the minute displacements of stellar lines which result from the star's motion with respect to the earth was next to impossible. It is therefore not surprising that Huggins' early results show but little agreement with those obtained by modern methods. The important fact remains that an immense field was opened up through these pioneering efforts.

The prosecution of this research at Greenwich, though persisted in for many years with inadequate facilities by Maunder, yielded few results of value. It was not until Vogel, whose first attempts had been made in 1871, again attacked the problem at Potsdam in 1887 that the great possibilities of the method were in any degree realized. Profiting by the advantages offered by photography, and employing a spectrograph especially designed for this research. Vogel substituted for the fluctuating and unsteady image seen at the telescope, a photographed image of the spectrum, accompanied by a comparison spectrum of hydrogen or iron. The plates, measured under a microscope in the most favorable laboratory conditions, yielded the first reliable determinations of radial velocity. With the exception of a single classical research by Keeler, who in 1890 measured with remarkable precision the radial velocities of the planetary nebulæ and certain stars by visual observations with the Lick telescope, all accurate determinations of this nature have been obtained by the aid of photography.

Once set on foot by Vogel, the study of radial velocities advanced rapidly. In 1898 a great step forward was made by Campbell, whose improvements of methods. embodied in the Mills spectrograph of the Lick Observatory, enabled him to secure results far surpassing in precision those previously obtained. After measuring the radial

velocities of several hundred stars in the northern heavens, he is now proceeding to South America to continue the work in the southern sky.

An important outcome of radial velocity determinations was the discovery of the so-called "spectroscopic binaries," which are double stars whose component members are too close together to be separately distinguished with any telescope. The rapid orbital motion of these bodies is betrayed by oscillations of the spectral lines. When only one of the bodies emits sufficient light to produce a spectrum, the only change observed is a to-and-fro motion of the lines, which are displaced toward the red while the star is moving away from us, toward the violet while the motion in the line of sight is in the direction of the earth. If both components are luminous, the lines of the compound spectrum are periodically doubled through the effects of the orbital motion. Some fifty spectroscopic binaries have already been discovered. So numerous are they that Campbell estimates them to comprise one-seventh of all the stars.

Researches on the radial velocity of stars with powerful instruments are in progress at the Lick, Yerkes, Potsdam, Meudon, Cambridge, Pulkowa and Cape observatories. At present most of the work is confined to the brighter stars, but it has been found at the Yerkes, Lick, and Potsdam observatories that the velocities of stars as faint as the eighth or ninth magnitude can be measured with fair precision with large telescopes. As the velocities of a great number of stars must be ascertained for use in studies of the structure of the universe, as well as in determinations of the motion of the solar system in space, it is evident that a system of coöperation between observatories engaged in the work should soon be arranged. Hitherto the necessity of developing methods and eliminating sources of systematic error has rendered independent work desirable, though certain standard stars are being systematically observed through mutual agreement of the above-mentioned observatories. Within a short time the period of preparation and experiment will have passed, after which a general plan of coöperation should be adopted.

Two important fields of stellar spectroscopy have yet to be noted. The study of the spectra of variable stars, particularly those of long period, for which important results have already been obtained by Fleming, Bélopolsky, Campbell, and others, promises an abundant harvest of results, if sufficiently powerful telescopes can be used for the purpose. In general, spectroscopic and photometric observations of these stars should be made simultaneously. The other

problem relates to the distribution in the heavens of stars of various spectral types. The extensive photographic surveys of the Harvard Observatory have provided the most abundant material for this study. The method of placing prisms over the telescope objective, employed by Fraunhofer and Secchi in their visual work, is here applied photographically. Thus the spectra of several hundred stars are obtained on a single plate. The entire sky has been photographed in this way. An examination of these plates has led to the discovery of a large number of objects of interest, among them six novæ, seven spectroscopic binaries, 75 stars of the fifth type, nearly 200 variable stars, besides 500 which occur in clusters. A detailed study has also been made of the spectra of the brighter stars north and south, several hundred lines being sometimes discussed in the spectrum of a single star.

Stellar Spectroscopes.—A brief consideration of the steps by which the instruments employed in stellar spectroscopy have been employed should be instructive in its bearing on further progress. The earlier spectroscopes gave small dispersion, and in most cases were merely the ordinary small instruments of the laboratory attached to telescopes When photography was first applied by Huggins it became apparent that special devices would be required to keep the star on the slit throughout the exposure. Hence his use of polished slit jaws, on which the reflected stellar image could be watched with an auxiliary telescope. The attempt to measure radial velocities made higher dispersion necessary and resulted in Vogel's specially designed spectrograph, in which freedom from flexure was an essential feature. It soon appeared, however, that if the prisms varied appreciably in temperature during the exposure, displacements of the lines would result. Hence Deslandres introduced apparatus for maintaining the prism box at a constant temperature, now universally employed. The success of the Mills spectrograph was due to no radical change in design or method, but rather to the use of a larger, stiffer, and more powerful instrument, improved in both optical and mechanical details, and employed under favorable conditions with a telescope of great light-gathering power. Frost's design for the Bruce spectrograph is a further step in the same direction. Each of the attachments for producing the comparison spectrum recently adopted at Mt. Hamilton and Potsdam embodies a distinct element of advance. In the new Chile spectrograph Campbell has introduced a radical modification of the mechanical design, for the purpose of reducing flexure. The optical parts

of this instrument differ in no important respect from those of the Mills spectrograph. The complete realization of laboratory conditions in stellar spectroscopy is the essential purpose of the experiments now being pursued at the Yerkes Observatory. A large grating spectrograph, rigidly mounted in a constant-temperature laboratory, will receive the star's light from a 30-inch cœlostat telescope. With this apparatus it should be possible to photograph the spectra of the brightest stars with a dispersion equal to that now employed for the Sun.

Spectra of Planets.—Excepting the objective prism, which can be used advantageously only with objects of small angular magnitude, the instruments employed in stellar spectroscopy can be equally well used in the study of nebulæ, planets, comets, and other heavenly bodies. The planets already afford a fruitful field of investigation, which promises to become more important in the future. As the light from the Sun reflected to us by the planets must pass twice through their atmosphere, the absorption thus produced can be detected in their spectra. In this way it has been found that Saturn possesses a dense atmosphere, which is absent or extremely rare on the rings. Perhaps the most interesting application of the spectroscope to the study of planets is the determination of their period of axial rotation. By proving spectroscopically that the matter composing the inner edge of Saturn's rings completes a rotation about the planet in a shorter time than the matter at the outer edge, Keeler showed observationally, as Maxwell long before had done theoretically, that the rings must consist of discrete particles. As soon as more powerful instruments become available, the disputed question of the rotation period of Venus will doubtless be settled by spectroscopic means.

Spectra of Nebulæ.—Comparatively few of the nebulæ have been investigated spectroscopically, and a most important field of research lies open here. The discovery of the terrestrial counterpart of the chief nebular line, the chemical and physical differences which characterize various parts of the same nebula, and the corresponding differences of radial velocity, are among the problems that most readily suggest themselves. The certain nature of the spectrum of such an object as the Great Nebula in Andromeda should be ascertained without further delay.

THE SUN

A complete knowledge of the phenomena and physical constitution of the Sun is of fundamental importance in connection with the

problem of stellar evolution. Every star in the universe is a sun, resembling in many respects the central body of our own system, but each representing a certain degree of development, from the nascent state, in close connection with the nebulæ, to the period of decline, exemplified in the red stars. With the exception of our own Sun, all of these stars are so distant from the earth that they appear in the most powerful telescopes as mere points of light. But with good atmospheric conditions a sharply defined image of the Sun a meter in diameter can be obtained, thus permitting the bright and dark spots upon its surface, and the flames at its edge, to be studied in their most minute details. A knowledge of the laws governing the variations of these phenomena is derived in part from statistical studies of photographs.

Solar Statistics.—The Royal Observatory at Greenwich, aided by auxiliary stations in India and Mauritius, has maintained a daily photographic registration of the visible phenomena of the solar surface on a scale of about 20 centimeters to the Sun's diameter. The data obtained from these plates, published annually in the *Greenwich Observations*, are sufficient to provide for investigations of the more conspicuous photospheric phenomena. These are supplemented by the "Sun-spot numbers," derived from the visual observations of many amateur and professional astronomers, which are published in the *Astronomische Mittheilungen*, established by Wolf, and continued by Wolfer, his successor at the Zurich Observatory. The prominences are observed visually with small instruments at Rome, Catania, Kalocsa, and Odessa, and the results, published regularly in the *Memorie della Società degli Spettroscopisti Italiani* and elsewhere, are suitable for a study of the distribution of the prominences in heliocentric latitude and longitude throughout the Sun-spot period.

In addition to prominences and spots which can be seen at the telescope, with or without the aid of the spectroscope, there are other solar phenomena of which a daily record is no less desirable. Scattered irregularly over the Sun's surface there are extensive areas of very hot calcium vapor, some of which coincide with faculæ. Though invisible to the eye, these regions can be photographed with the spectroheliograph, which records all phenomena characterized by brilliant emission of the K line of calcium. Thus the chromosphere and prominences also appear on suitably exposed plates taken with this instrument. A daily record of solar phenomena was maintained with the spectroheliograph at the Kenwood Observatory,

in Chicago, from February, 1892, to May, 1895, after which the instruments were removed to the Yerkes Observatory. On account of the necessity of remodeling the Kenwood telescope for other observations, this series of observations has been discontinued. Many photographs of the Sun have been taken, however, with a spectroheliograph attached to the 40-inch Yerkes telescope, and a daily series will soon be resumed as part of an extended programme of solar investigations, for which the necessary instruments are nearly completed. Spectroheliographs are also in service at the astrophysical observatories of Meudon and Potsdam, and a committee of the Royal Society is about to have one constructed for use in India.

Janssen has obtained directly enlarged photographs of Sun spots and photospheric granulations on a scale great enough to show the structure admirably. This work is still in progress at the Meudon Observatory, and some equally good results have been obtained by Bélopolsky at Pulkowa. It is important that further experiments of this kind be undertaken at other observatories, and that a systematic record of the changes in spot and atmospheric structure be maintained.

It will be seen from what has been said that when all of the new instruments are in use, the provision for recording the forms of solar phenomena will be nearly sufficient. The photographic record of Sun spots made under the direction of the Greenwich Observatory need not be duplicated elsewhere, at least in so far as the systematic reduction of all the plates is concerned. For comparison with spectroheliograph plates, however, direct photographs should be taken at other observatories. The small size of the solar image used at Meudon and at Potsdam (and planned for the Indian spectroheliograph) precludes the possibility of photographing the smaller details of the chromosphere. For this reason a large scale record to supplement that of the Yerkes Observatory should be provided for at some tropical station. For studies of special phenomena an even larger image could be used to advantage, and large-scale direct photographs of spots should be taken for comparison. The work of photographing prominences simultaneously in lines of different elements, such as calcium and hydrogen, which has already yielded some interesting results at the Kenwood Observatory, should also be continued. There are many other special investigations to be made with the spectroheliograph.

Spectroscopic Observations.—The solar spectrum has been studied with epoch-making results by Rowland and his associates at the Johns Hopkins University, and in the infra-red region by Langley and Abbot at the Smithsonian Astrophysical Observatory. The marked success of Higgs in photographing the extreme red on plates prepared by himself points to the possibility of extending this work to the infra-red, a result greatly to be desired. All of this work relates to the integrated light of the Sun, and not to any particular point on its surface. Our knowledge of the spectra of Sun spots is derived almost entirely from visual observations made with the spectroscopes of twenty years ago, and is purely qualitative in character. An adequate study of the characteristic widened lines, which may involve the application of photographic methods, should be based upon measurements of both wave-length and intensity. A large solar image and a powerful spectroscope are indispensable for this work. The following programme for the study of Sun spots may serve to illustrate the opportunity for research in this field:

	Size of image.	*Purpose.*
(1) Photograph of form (direct) ..	17 cm.. ...	To identify spot and give general form and heliographic position.
(2) Photograph of form (enlarged)	40 to 100 cm.	To record details of structure.
(3) Photograph of form (in calcium light).	17 cm.. ..	To show distribution of calcium vapor for comparison with (1).
(4) Photograph of form spectrum.	17 cm.... .	To record widened lines.
(5) Photograph of form (H and K lines, with very high dispersion)	17 cm......	To show radial motion of calcium vapor.
(6) Heat measurement with bolometer or radiometer.	40 to 100 cm.	To give heat radiation, compared with that of neighboring photosphere and center of the Sun.

The slit used in photographing the spectrum and in measuring the heat radiation is to be set successively at various points in the spot, which are recorded on the photograph of its form.

It would be easy to name many similar investigations of spots, faculæ, and prominences, which would be certain to add greatly to our knowledge of the Sun. It is perfectly feasible, for example, to study some of the most important phenomena of the reversing layer without an eclipse; indeed, many lines which are apparently too faint to be photographed at eclipses in the spectrum of the "flash"

have been repeatedly observed at the Yerkes Observatory in full sunlight. It should not be forgotton, moreover, that the prismatic camera or objective grating, though perfectly adapted for eclipse work, may give results which are not always capable of certain interpretation. Thus a faint chromospheric arc, whose depth is sufficient to require some time for the Moon to pass over it, may produce in a photograph an effect similar to that given by a much shallower but more brilliant stratum. With sufficiently powerful apparatus such questions could be easily settled by observations made in full sunlight. A large grating spectroscope (for many such purposes the astigmatism of the concave grating need not seriously interfere with its employment), used with a cœlostat and a long focus objective or speculum giving a large focal image of the Sun, would probably bring to light many new phenomena, and permit those now known but not yet understood to be rigorously investigated.

From recently published photographs it appears that at rare intervals the Sun's reversing layer is temporarily so completely changed in character as to render the solar spectrum corresponding to the disturbed region almost unrecognizable. In order to secure observations of other such phenomena—the only one so far recorded lasted only a few moments and involved a region whose length was at least one-sixth of the Sun's diameter—the solar spectrum corresponding to the most active regions of Sun-spots should be kept continuously under observation during the spot maximum. A series of photographs of the spectrum, taken at brief intervals of time, may be needed for this purpose. On account of the large number of photographs required to give a fairly continuous record, some plan of coöperation ought to be adopted. A single photograph of such a phenomenon would well repay all the time and trouble required to obtain it.

As the field of solar research is almost unlimited, many other problems requiring investigation will suggest themselves. If modern methods had already been applied to the solution of solar problems, thus diminishing the chances of securing new and striking results, a reason, though an inadequate one, for the present neglect of solar research might be found; but, with a few notable exceptions, such applications have yet to be made.

The Advantages of Reflecting Telescopes.

During the nineteenth century the development of the telescope in all countries except England was confined almost exclusively to

the refractor. In England the evident theoretical advantages of the reflector and the comparative ease of making large speculæ led during the first half of the century to the construction of larger and larger instruments of the reflecting type, which culminated in Lord Rosse's great six-foot reflector, erected at Parsontown in 1845. The crudeness of the mounting provided for this great mirror, due to the necessity of constructing it without the advantages afforded by modern engineering methods, did not prevent the Irish astronomers from securing many important results; but even had sensitive photographic plates been available at that time, the advances made in later years could not have been achieved, for a large mirror or objective, however perfect, is of comparatively little value unless provided with a mounting and a driving clock of accurate workmanship and suitable design. The absence of such mountings undoubtedly delayed for many years the recognition of the advantages of reflecting telescopes.

After the completion of Lord Rosse's reflector, attention seems to have been concentrated in large measure on the development of refracting telescopes. Notable exception should be made of Sir William Huggins, who fully perceived the advantages of the reflector in spectroscopic research and employed such an instrument exclusively in his classic investigations; but through the increased skill of the makers of optical glass and the genius of such men as Alvan Clark, the refracting telescope grew rapidly in size and perfection. The optical improvements were accompanied by corresponding advances in mechanical design, and great perfection has been attained in such modern instruments as the great refractors of the Potsdam, the Lick, and the Yerkes observatories.

Meanwhile the introduction of photography into astronomy and the rapid improvement of photographic processes had revolutionized observatory methods. In a period of rapid development the merits of the reflector, so obvious from an astrophysical standpoint, could not remain long overlooked. In 1883 Draper secured with a refractor the first photograph of the Great Nebula in Orion. The first to make extensive use of the reflector for the photography of nebulæ was Roberts, whose results form a record of great value. The construction of large reflectors was again undertaken. Special reference must be made to the work of Common, who not only constructed mirrors of three and five feet aperture, but provided them with improved, though hardly adequate, mountings and invented the double-slide plate-carrier. This simple device, which so greatly

reduces the difficulty of maintaining the image of a celestial object at a fixed point on the photographic plate, has proved to be of great importance. With its aid Common secured excellent photographs of the Great Nebula in Orion.

The high opinion of the possibilities of the reflecting telescope which is entertained by astronomers at the present time is based in large degree upon the results obtained by Keeler with the Crossley reflector of the Lick Observatory and by Ritchey with the two-foot reflector of the Yerkes Observatory. The Crossley reflector, with three-foot mirror by Grubb, is not provided with a thoroughly modern mounting.* In ordinary hands it is not improbable that photographs representing no marked advance would have been obtained with it; but through Keeler's skill the instrument was made to yield results of unexpected excellence. Few spiral nebulæ had previously been known to exist, but Keeler's photographs showed them to be as numerous as all other forms combined; indeed, they probably represent the type object to which our present ideas of the nebular hypothesis must be made to conform. Without the aid of a large reflector in competent hands it is doubtful whether this fundamentally important discovery would ever have been made.

Ritchey's results, though obtained with an instrument of only two feet aperture, constructed in the shops of the Yerkes Observatory, leave no element of doubt as to the great possibilities of the reflector. With this small instrument stars too faint to be seen or photographed with the 40-inch Yerkes telescope (the largest refractor hitherto constructed) can be photographed in forty minutes. The faintest star within reach of the 40-inch telescope is of the seventeenth magnitude, but with the two-foot reflector photographs made with exposures of six or seven hours show stars which are estimated to be one or two magnitudes fainter. The advantages of the reflector are still more striking in the case of the nebulæ, especially for such objects as the Great Nebula in Orion, the Great Nebula in Andromeda, the nebulæ in the Pleiades, the spiral nebulæ—indeed, for all such objects except the minute planetary nebulæ, which would also be shown to advantage by a reflector of great focal length. Furthermore, measurements of stellar photographs made with the Crossley three-foot reflector show that if the field employed is not too large the positions of stars can be determined with precision. For spectrographic research the reflector offers great advantages over the refractor, especially for work in the hitherto almost unex-

* Such a mounting is about to be constructed.

plored region of the ultra-violet. For these reasons astronomers are agreed that the construction of a large reflecting telescope at the present time would open up many fields of investigation, in which results of great value would undoubtedly be obtained.

Such a telescope, which would greatly surpass all existing instruments in power, should be used first in the northern hemisphere, and subsequently south of the equator, where the southern heavens, as yet unexplored with comparable optical power, offer limitless possibilities of discovery and research. With it there could be obtained a series of large scale photographs of the nebulæ, which would serve as standards of reference in years to come for the detection of evidences of motion or development. In conjunction with spectrographs of moderate and high dispersion, the latter mounted on fixed piers in constant-temperature laboratories, it would be possible to study stellar spectra and to trace the evidences of stellar evolution in a manner quite beyond existing means. Variable stars, which at their period of minimum are too faint to be seen in the most powerful instruments, could be followed through all their phases. The spectra of variable stars, which have been but little studied, could be investigated to the greatest advantage. Another important use of a large reflector would be in the measurement of the heat radiated from the stars. For this work a refracting telescope is useless, but the researches on stellar heat by Nichols at the Yerkes Observatory with a small reflector shows that much could be done with a large instrument of this type.

Respectfully submitted.

GEORGE E. HALE.

APPENDIX D TO REPORT OF COMMITTEE ON ASTRONOMY.

THE QUANTITY AND NATURE OF SOLAR RADIATION.*

BY S. P. LANGLEY.

The study of the Sun may be pursued from two standpoints, in a measure distinct from one another. On the one hand we may regard it as the nearest and therefore most readily studied of the stars, and seek to know its materials and their arrangement and reactions, in order to apply the results so gained to enlarge our views of the

*Prepared in answer to request from Advisory Committee on Astronomy of the Carnegie Institution, May, 1902.

universe; and, again, we may study the Sun as the source of that radiation which makes the Earth habitable. In this case we have a problem which concerns all humanity and all life on Earth, and one, however, strangely neglected, which is incomparably the most important to man of anything which Astronomy has to offer.

Our concern in this case is to determine how large an amount of radiation the Sun emits; what is the nature of these rays; what modifications they undergo in their passage through the Sun's envelope and the Earth's atmosphere; what, if any, variations occur in the total radiation of the Sun, and all this with final reference to the effects of these variations on life upon the Earth. The abstract and the utilitarian interests are closely joined, but it is chiefly with reference to the latter that the following remarks are offered.

The total radiation of the Sun, usually measured by the heating effect of the rays falling perpendicularly upon a square centimeter of area, has been determined by numerous observers at many stations on the Earth's surface. Many of these measurements have been corrected according to more or less plausible theories of the absorption of the Earth's atmosphere. The result so reached, which purports to be the heat equivalent of the solar radiation falling perpendicularly upon a square centimeter of surface outside the Earth's atmosphere, is termed the "solar constant." Whether or not the name is well chosen is doubtful, for we do not yet know, after a hundred years of careful actinometry, even within wide limits, whether the emission of the Sun is constant or variable. Indeed, if we should trust the experimental results implicitly, we should regard the "solar constant" as one of the most variable things in nature; for the most authoritative values range from less than 2 calories up to over 4 calories per minute.

The great discrepancy in these results does not necessarily imply an actual variation in the amount of solar emission, but is chiefly caused by differences in the method of reducing the observations to eliminate the absorption of the Earth's atmosphere. Thus it was for a long time customary to treat the atmosphere as if one portion of it were just like another in its absorption. Upon this basis, if, for example, a layer thick enough to exert a barometric pressure of 1 decimeter transmitted A per cent of the incident beam a second

* To prevent confusion, it may be stated that a solar constant of 3 calories per minute indicates that the heat equivalent of the solar radiation falling perpendicularly upon a surface of 1 square centimeter area for 1 minute of time is 3 times the amount required to raise the temperature of 1 gram of water through 1 degree centigrade.

layer exerting the same pressure would transmit the same percentage A of the beam, so that after passing the two layers the percentage A^2 would remain. This argument, which was that of Pouillet, leads to serious error, for it is now a well-determined fact that the layers of the air do not possess uniform properties of absorption and do not possess a uniform transmission coefficient for all wave-lengths.

It has thus far proved, and, so far as can be seen, always will prove, impossible to determine from near sea-level with any precision by any observations, however careful or long continued, the "constant" of solar radiation. There is no good way to eliminate the complex effect of atmospheric absorption except to observe at the highest practicable altitude, preferably near the tropics, but most certainly in a dry and clear atmosphere, and preferably where there are two stations in view of each other, the first of which is at a notably greater altitude than the second, though the latter is itself at least some thousands of feet above sea-level.

Temporary expeditions with meagre outfits have gone from time to time to high mountain stations for solar observations, and small meteorological stations have even been longer continued. What is needed is rather a permanent astrophysical observatory equipped with the most powerful and refined modern apparatus for solar research and located at the highest and clearest station it is practicable to occupy.

The remarkable complexity of atmospheric absorption has been hinted at in what has just been said, but much remains unsaid and even unknown in this connection. Hardly less importance attaches to the absorption of the air for the radiations emitted by the Earth than to its absorption for the rays received from the Sun, and this part of the subject has been relatively unknown and till recently beyond the possibility of thorough study. It is apparent, however, that the nice balance of the receipt and outgo of radiation by the Earth is that which maintains the range of climate under which we live, and that a thorough understanding of the matter requires the study of the outgo as well as that of the incoming of radiation.

It is for this purpose and for extending our knowledge of the direct absorption of the solar rays in our atmosphere, and even largely for the determination of the solar constant itself, that the station at a relatively low altitude, but within sight of the elevated station, and which should be provided with a duplicate set of apparatus for energy of radiation work, is needed.

It has been surmised within recent years that the amount of radiation of the Sun is variable, with an average period of approximately eleven years, corresponding with the period of maximum frequency of Sun spots. It has thus far been impossible to determine this, and the possible change in the solar atmosphere, because of the variability of absorption of the terrestrial atmosphere. This subject should form an important part of the investigations to be undertaken by a supposed high-altitude observatory. The writer has long believed and said that a variability of the absorption of the solar envelope is a probable cause of the (probable) variability of the solar radiation. Investigation of the solar atmosphere should therefore go on at the same time as that of the Earth, and this incidentally gives another need for the high station, for this part of the work demands such a large and motionless solar image as can rarely be obtained in our lower atmosphere.

I have said little of the instrumental means for these principal objects, but they would be largely fitted to thermal studies. These studies would be associated with daily photographic records of the face of the Sun, and perhaps magnetic records and investigations into the emanation of X-rays and others. The barest suggestions of what may be done in the illimitable field are here given.

To conclude, these studies are utilitarian in the highest sense, for though we may never hope to affect the original source of solar radiation by any human effort, there is every hope that we may learn to forecast its effects upon the earth and provide for them.*

S. P. LANGLEY.

APPENDIX E TO REPORT OF COMMITTEE ON ASTRONOMY.

PRESENT STATE AND NEEDS OF ASTRONOMICAL RESEARCH.

BY SIMON NEWCOMB.

Of the two great branches into which astronomical research is at the present time divided, astronomy proper and astrophysics, the latter has been so fully treated in the papers submitted by Professors

* See letter of February 28, 1902, addressed to Mr. C. D. Walcott, Secretary of the Carnegie Institution, printed on page 110.

Pickering and Hale that it need not be considered in the present one. The following remarks therefore refer mainly to the branch of celestial measurement sometimes termed *astrometry*.

It must not be inferred that these two branches are really distinct. The forces of both have to be combined in working out the great problems of astronomy.

At the outset it is necessary to consider certain features in which astronomy is radically different from other branches of science, and which render it inadvisable to apply to its promotion any general rules derived from experience in the case of other sciences.

I. General Character of Astronomical Research.

Astronomy differs from other physical sciences in being founded entirely on observations, while the others rest mainly upon experiment.

Its laws and phenomena are of slow development, generally in cycles of years, centuries, or ages.

Their complete investigation involves difficult and complex mathematical problems, the numerical computations connected with which are long and arduous.

For all these reasons important astronomical results generally require a combination and comparison of observations continued through long periods of time.

In its relation to the welfare of mankind astronomy is also peculiar. In addition to its immediate connection with that welfare, astronomical research may bring to light cosmical processes pregnant with the destiny of our race.

These same circumstances render a different kind of genius necessary in the case of astronomy and of the sciences of experiment. The skill required in an astronomical investigator is not that of the experimentalist, but of one having such a grasp of the mathematical relations involved in the subject which he is treating that he can devise methods which most economically and surely lead to the desired results.

In working out these results a great amount of computation or observation is generally required. This part of the work can generally be done by routine computers, who are not necessarily masters of the great problems of astronomy, but who simply follow the formulæ supplied them.

II. Two Features of Astronomical Research at the Present Time.

The first feature is a certain one-sidedness of astronomical research as it has hitherto been pursued. Excluding private establishments, here and there, managed by their owners—institutions for instruction only, and inactive observatories—there remain fifty or sixty observatories of a more or less public character, supported by governments, universities, or special foundations, supposed to be in a state of astronomical activity. A rude estimate of the probable annual cost of these institutions foots up more than half a million dollars, and might approximate to a million. They are concerned almost entirely with the making, reduction, and publication of their current observations, while the important work of combining these observations and those made by our predecessors, so as to obtain from them the best ultimate results for astronomy, is comparatively neglected. The few institutions which do more or less work in this line occupy themselves mainly with special branches, and not with any comprehensive combination of the whole. Moreover, a comprehensive discussion and combination of the observations of the past is beyond the power of one man or of any existing organization.

The other feature is that the astronomical agencies of the world are independent organizations, and that each has generally worked in its own way with little reference to what is being done by others. At present there is a marked tendency to coöperation and unification, but these processes are slow.

The same remark applies to the mathematical researches necessary to the development of the science. They are made sporadically by isolated individuals, each using the method which he chances to have worked out or learned.

In view of recent progress toward international unification and coöperation, the time seems ripe for a general scheme for deriving the best results from the astronomical work of the past and present.

To gain the highest utility from a given amount of labor it is necessary that the latter be applied upon a comprehensive plan devised with especial reference to the ends ultimately in view. The following survey of the field may be regarded as an attempt toward the conception of such a plan. Emphasis will be laid on those subjects which require combination and coöperation, while subjects which individuals are able to deal with will not be dwelt upon.

III. The Researches Now Most Needed

The heavenly bodies, to the investigation of which astronomical research is directed, are of two classes, the one comprising the bodies of the solar system, the other the fixed stars

With respect to the first, it may be said that such subjects as the phenomena presented by the sun and the question of the physical constitution of this body, the aspects and rotation of the planets, the nature of the comets, and celestial physics generally either require only individual effort or are, in most cases, adequately provided for. Cases in which an investigator is in urgent need of more help than he commands can be dealt with singly as they arise.

In what concerns the motions of the planets it is believed that the existing tables satisfy all requirements.

The case is quite different with the satellites The want of new tables of the moon is one of the most urgent in our exact astronomy, and one of the most difficult to supply. This difficulty arises from the two facts that the problem of the moon's motion is the most complicated of mathematical astronomy, while the amount of labor involved in carrying out any solution of it is greater than in the case of any other heavenly body. The promotion of the necessary preliminary research and the construction of new tables of the moon's motion, therefore, seem to me of the first order of importance.

Tables of the satellites of Jupiter are also greatly needed. It is believed that one, or perhaps two, competent astronomers are ready and desirous to undertake the preparation of such tables, but are deterred by the amount of labor involved and the difficulty of the problem.

Most interesting problems are afforded by the satellites of Saturn and Mars. It is believed, however, that these can be adequately dealt with by individuals, and therefore need no promotion by the Carnegie Institution. The same remark will apply to the satellite of Neptune. The writer is engaged in a discussion of the observation of this body, but needs no additional assistance.

A question of transcendent importance in astronomy is whether the force of gravitation varies exactly as the inverse square. There is now strong reason to suspect that such is not the case. If a deviation can be fully established, its explanation may open up a new field in physics. The work bearing on this question, which is not in regular progress, and yet requires most urgently to be done, is a

comparison of the best tables of Mars with all the good existing observations of that planet. This work is beyond the power of an individual, and is, therefore, of the class which can most properly be supported by an institution.

Passing now to the fixed stars, the foundations of a new science of stellar statistics are being laid. The pursuit of this science requires a combination of all the observations and methods of stellar research in both the fields of astrometry and astrophysics. To the very able and comprehensive survey of the subject by Professor Boss I wish to add the following considerations:

It seems to me essential that whatever aid the Carnegie Institution may give to researches in this class should be so applied as most effectively to gain the desired end. Among these ends that which must take the first place, so far as quantity of labor and need for help are involved, is the determination of the motions of as many stars as possible in every part of the sky. For this purpose it is essential that the researches of Professor Boss be supplemented by a re-reduction of certain series of former observations. The necessity of this arises from the fact that the proper motion of a star can, in most cases, be determined only by a comparison of observations as widely separated as possible.

The observations which most need re-reduction are the following:

1. The observations made at the Royal Observatory, Greenwich, by Maskelyne, from 1765 to about 1816. These observations have been neglected, owing to their having been made with an imperfect instrument, but it is believed that, with but little labor, they could be so reduced as to be made useful to astronomy. It would seem, in fact, that the office of the American Nautical Almanac was able to reduce most of them in a provisional way with the very limited means at its disposal. But it is desirable to have the work carried through more thoroughly.

2. The Greenwich observations by Pond, especially those of declination, have been shown by Chandler to be of the highest degree of precision. Those which he has provisionally reduced already form a valuable contribution to exact astronomy. It seems desirable that the work of reducing them should be completed.

3. The observations by Piazzi at Palermo, about the end of the eighteenth and beginning of the nineteenth century, are celebrated in the history of astronomy. Their re-reduction has been one of the desiderata of astronomy for half a century and the necessity for it has frequently been pointed out. Quite recently the work has been

seriously undertaken by Professor Porro at Turin and Dr. H. S. Davis in this country. The latter has been aided by the Trustees of the Gould fund. The question whether anything to promote the work can be done by the Carnegie Institution is respectfully submitted.

4. Attention has recently been called by Professor Rambaut to a remarkable series of observations made by Professor Hornsby at the Radcliffe Observatory, Oxford, beginning in 1774, which he is desirous to reduce. From his report it would seem that the results may be of special value. It therefore seems desirable to investigate the question of their precision and of the character of the results to be derived from them. When this is done the question of providing for their complete reduction could be considered.

Respectfully submitted.

SIMON NEWCOMB.

WASHINGTON, *October 8, 1902.*

APPENDIX F TO REPORT OF COMMITTEE ON ASTRONOMY.

PROGRESS AND PRESENT STATE OF CELESTIAL PHOTOMETRY AND PHOTOGRAPHY.

BY E. C. PICKERING.

CELESTIAL PHOTOMETRY.

The earliest estimates of the relative brightness of stars are recorded by Ptolemy in the *Almagest* but careful comparisons, capable of revealing small changes in brightness, were first made by Sir William Herschel. The inadequacy of visual estimates was recognized by Sir John Herschel while engaged in extending and improving his father's work. This led to the constructions of the first stellar photometer, by which the light of a star was compared with a minute image of the Moon, adjustable in distance from the eye. The full possibilities of comparisons made without instrumental aid for large numbers of stars were realized by Argelander, whose *Uranometria Nova*, giving the approximate magnitudes and places of 3,256 stars, was published in 1843. This was followed twenty years later by the monumental *Bonner Durchmusterung* giving the positions and magnitudes of 324,189 stars in the northern heavens. In the prepa-

ration of which Argelander was aided by Schönfeld and Krüger. Schönfeld's *Southern Durchmusterung*. Thome's *Cordoba Durchmusterung*, and Gill and Kapteyn's *Cape Photographic Durchmusterung* have done a similar and even greater service for the southern heavens. But neither visual estimates nor photographic determinations can supply the precise measures of stellar magnitudes obtainable with the aid of a suitable photometer.

Reference has already been made to Herschel's stellar photometer, with which 69 stars were observed. These results are valuable mainly as pioneer efforts. But the measures of 208 stars made during the years 1852–1860 with a Steinheil prism photometer by Seidel were of a higher degree of precision. Zöllner's photometer, in which an artificial star is reduced to the intensity of the observed star by the aid of a polarizing prism, is described in his *Grundzuge einer allgemeinen Photometrie des Himmels*, published in 1861. This work also contains photometric observations of over 200 stars, which were made more for the purpose of testing the photometer than for the formation of a catalogue of magnitudes. Zöllner's photometer was first systematically used for this purpose at the Harvard College Observatory by Peirce, whose catalogue gives the magnitudes of 495 stars lying between + 40° and + 50°. Wolff's two catalogues (1877 and 1884) of stellar magnitude determined with a Zöllner photometer contain over 1,100 stars. Reference should also be made to the excellent work of Lindemann on the magnitudes of stars in the Pleiades and his revision of the magnitudes of the Bonn *Durchmusterung*, and to that of Ceraski on the magnitudes of circumpolar stars.

The first observatory which made the measurement of the light of the stars an important part of its work was that of Harvard College. In 1879 the meridian photometer, an instrument devised with special precaution for the elimination of systematic errors, was used to measure the light of 4,260 stars. Among them were included all stars which were of the sixth magnitude or brighter, according to any well-known authority, and north of declination —30°. When published in 1884 it included the reduction to the photometric scale of the magnitudes of all the stars contained in the principal catalogues preceding it. The *Almagest*, A. D. 138, and Sufi, A. D. 964, were included, also the six catalogues of Sir William Herschel, which furnish determinations of the light of 2,785 stars a century ago with an accuracy not again attained for more than fifty years.

The *Uranometria Oxoniensis*, which contains the measures of 2,647 stars north of declination —10°, made by Pritchard at Oxford with

a wedge photometer, was published in 1885. In 1886 Müller and Kempf at Potsdam entered upon their important investigations in stellar photometry. Their first work involved the precise measurement of the light of all northern stars in the *Durchmusterung* of the magnitude 7.5 and brighter. A Zöllner's photometer, embodying many improvements, was employed. The first portion of this valuable work, which was published in 1894, contained measures of 3,522 stars between declinations 0° and + 20°. The second portion contained measures of 4,416 stars between declinations + 20° and + 40°, and was published in 1899. Meanwhile, at the Harvard Observatory, a new and larger meridian photometer had been constructed, with which more than a million photometric settings have been made. All stars of the magnitude 7.5 and brighter in the *Durchmusterung* and north of declination — 40° have been measured, and the stars of the *Harvard Photometry* remeasured; also about 17,000 stars, mainly of the eighth and ninth magnitudes, arranged in zones at intervals of 5° in declination. All of this work has been extended to the South Pole by the establishment of a station in the southern hemisphere at Arequipa, Peru. The number of faint stars is so enormous that it does not seem advisable to attempt to measure them at all. The observations of Herschel show that slow changes of long period occur rarely, if at all, among the brighter stars. It is doubtful, therefore, if the meridian photometer need be kept actively at work, except for the purposes named below in connection with variable stars, and for measuring other standards. Of course, at intervals the brighter stars should be measured, and any tests of systematic errors affecting the scale established with this instrument will always be of value.

For measuring the faint stars much remains to be done. At the Harvard Observatory a 12-inch telescope has been mounted horizontally and a photometer attached to it, so that stars as faint as the thirteenth magnitude can be measured. The work is made purely differential by always measuring certain stars already measured with the meridian photometer. About 250,000 settings have been made with this instrument during the last four years. The photometric scale has been extended to magnitude 10.5 by measuring a series of zones of *Durchmusterung* stars, 10′ wide and 10° apart in declination. The photometric scale is now being extended to the faintest stars visible in the largest telescopes by the coöperation of the Yerkes, Lick, McCormick, and Harvard Observatories. By a small appropriation from the Rumford fund and the friendly coöperation of the

directors of these institutions, the use of telescopes of 40, 36, 26, and 15 inches has been secured for a single investigation. The two largest telescopes in the world are included in this work. The standards for measurement have been selected from the vicinity of known variable stars of long period. Charts of a large number of these regions have been constructed by Father Hagen, on which all the stars of the thirteenth magnitude and brighter have been entered. Thirty-six of these regions have been selected, approximately at equal intervals, but all north of declination —30°. In each of these regions five stars of the twelfth, fourteenth, fifteenth, and sixteenth magnitudes are selected as standards. The twelfth-magnitude stars have been measured with the twelve-inch telescope mentioned above. Good progress has been made in measuring the fainter stars with the larger telescopes. It is hoped that in two or three years we shall thus be able to furnish standards of magnitude for the faintest stars visible in the largest telescopes upon a uniform photometric scale.

It will be seen from the above statement that the photometry of the brighter stars is better provided for than are some other departments of astronomy. For the faint stars a beginning only has been made, and we have no means of judging whether the methods we are using are good. The use of one or more telescopes of at least two feet aperture is much to be desired. Other methods should also be employed to check systematic errors.

Various attempts have been made to determine the photographic brightness of the stars. The first method, that of measuring the diameter of the images, proposed by Bond in 1857, has perhaps been more generally used than any other. Methods have been tried depending on variation in aperture and exposure and on the use of polarized light. If this problem can be solved, it may supersede visual observations of magnitude. The fact that red stars photograph faint is rather an advantage, since a comparison with visual magnitudes gives also a definite measure of the color of the star. The only complete solution of the problem of determining the colors of the stars or other sources of light is by measuring the intensity of different portions of the spectrum. This may be done much more easily by photography than visually. One of the principal objects of photometry is to determine the distribution of the stars. Again, photography has a great advantage over visual observations, especially in the case of faint stars. If possible, the stars should be classified according to their physical condition as shown by their spectra. In nearly all of this work the ultimate comparison is the

estimate of equality by the eye. Many of the errors thus introduced may be eliminated by substituting for the eye a radiometer, bolometer, or thermopile and measuring the proportion of heat intercepted by an image on a photographic film. Another piece of work which is sadly in need of revision in accordance with modern methods is the precise determination of the relative brightness of the Sun and other members of the solar system as compared with the stars.

Variable Stars.—One of the most important applications of photometry is to the study of stars which vary in light. Variable stars may conveniently be divided into new stars, variable stars of long period, those having small and irregular variations, those of short period, and the Algol stars.

There is no obvious way of aiding the prompt discovery of new stars, and when such an object is found the general interest in it insures an abundance of observations. Except in the case of stars bright enough to be visible to the naked eye, all recent new stars have been found by the aid of photography, and nearly all of our knowledge of them has been found by photographic processes.

The study of variable stars of long period is one that can be greatly aided by judicious appropriations. The laws regulating the changes in these bodies are unknown and appear to be complex. They can be determined only from long series of observations extending over many years. If not made now we may be blamed by future astronomers who may justly say, "How much we could do now if our predecessors had not neglected their opportunities." Each star should be observed at least once a month. The observations are easily made, require but little apparatus in addition to a telescope, and much valuable work has been done on them by amateurs. Many of the large telescopes of the country are now idle during a great portion of the night, for want of the means of employing a competent observer. A small sum would pay the salary of an advanced student who might thus at the same time continue his studies and do useful work. Careful superintendence and coöperation would be required to insure a proper distribution of the observation of each star, otherwise as at present some stars would be neglected, others observed more frequently than is necessary. The same comparison stars, the same system of photometric magnitudes, and the same form of publication should be used by all the observers.

The observations of the variable stars of short period stand on a wholly different basis. These stars appear to vary with perfect uniformity, except for slight variations in the length of the period. It

is therefore only necessary to determine once for all the nature of the variation, and then occasionally determine the times of maximum light to see if the period has undergone any change. The variations can be determined so accurately by photometric measurements that visual observations are of little value. The light curves of a large part of them have already been determined at Harvard, and it is now only necessary to measure these stars from time to time. Some of them vary so rapidly that the periods of variation can be found with great accuracy. An attempt is made at Harvard to measure each star which changes more than half a magnitude an hour, on three nights each year when it is increasing in light, and on an equal number when it is diminishing. The same remarks apply with even more force to stars of the Algol type. If all the light curves can be determined once for all, occasional observations only will be needed later.

We may therefore conclude that appropriations are much needed to secure the required observations of variable stars of long period, but that sufficient provision is now made for the photometric study of other variable stars. Spectroscopic observations of such stars, which are greatly needed, are referred to in the section on spectroscopy.

Celestial Photography.

The application of photography to astronomy by Bond in 1850, and again in 1857, was an advance comparable in importance with the invention of the telescope. Bond's experiments were made with a visual telescope ill-adapted for the work. The production of the first photographic refractor by Rutherfurd in 1864 enabled him to obtain many remarkable photographs of the Sun, Moon, and stars. His negatives were used by Gould in 1866, and later by Jacoby and others in the precise measurement of such stellar groups as the Pleiades In 1880 Draper obtained the first photograph of the Great Nebula in Orion. Up to this time the use of wet plates had greatly hampered the work, but the introduction of the much more sensitive dry plates led at once to important advances, especially at the Cape, at Paris, and at Harvard. At the Cape of Good Hope, Gill photographed the great comet of 1882 with an ordinary portrait lens strapped to a telescope. The immense number of stars recorded on the plate led to the publication in 1896 of the *Cape Photographic Durchmusterung*, which gives the approximate positions and magni-

tudes of 454,875 stars, nearly all south of declination —18°, as measured by Kapteyn on Gill's plates. This great work is memorable not only as the first of its kind, but for the promptness with which it has been completed and published. The successful results of the MM. Henry at the Paris Observatory led to the formation of a permanent international committee for making a photographic chart of the sky. At the first international congress held in Paris in 1887, seventeen different nationalities were represented. A gigantic plan was formed in which eighteen observatories took part. Each agreed to have a 13-inch telescope constructed, and to photograph a certain zone with a short and a long exposure. A catalogue was to be formed from measures of all the stars shown on the plates taken with short exposures, giving about a million and a quarter stars to the eleventh magnitude. The long exposure plates, containing perhaps twenty million stars to the fourteenth magnitude, were to be published in the form of charts. The Potsdam Observatory has published two quarto volumes of about 500 pages each, giving the results of measurements of 57 and 38 plates. They give the precise rectangular coördinates of each star, but, owing to the great expense of the reductions and the lack of suitable meridian circle positions, the values of the right ascension and declination are only approximate. Three hundred of the charts have been published. The size is 28 x 28 centimeters and the scale 30 seconds to one millimeter. Eleven thousand of these plates would be required to cover the whole sky. Although fifteen years have elapsed since the first conference, the amount of material published by no means represents the amount of work done. No zone has been taken by any observatory in the United States. Including the very heavy cost of measurement and reduction, it is doubtful if the entire work could be completed at a total expenditure of $3,000,000. This great undertaking, if ever completed, will furnish a record of the state of the heavens upon which the work of centuries may be based. The plan adopted at the Harvard Observatory in the early eighties recom-, and mended to the first Paris conference in 1887, was that all the photographs should be taken with a single photographic doublet. Each plate taken with the Bruce 24-inch telescope, completed in 1892, which is of this type, covers a field of 25 square degrees, as compared with the four square degrees covered by the international charting telescopes. The errors in such plates due to distortion have been shown by Professor Turner to be insensible. The entire sky in either hemisphere can be charted with this instrument in one

or two years. The Draper telescope in Cambridge and the Bache telescope in Arequipa, Peru, are of this type, and have been in constant use by the Harvard College Observatory during the last thirteen years. The apertures are eight inches, and each covers a field 10 degrees square. The resulting collection of photographs represents each portion of the sky on from one to two hundred nights, and furnishes a historical map of great value. It has not seemed advisable to go to the expense of measuring and cataloguing a large portion of the fainter stars, but when an object of interest is discovered its history is always readily available, as has been shown in many striking cases. Prints on glass of any particular plate in this series are furnished to those who may require them for special studies. There can be no doubt that a systematic examination of these plates would lead to many important results.

Photography of Nebulæ.—The recent development of the reflecting telescope, referred to more specifically in another section of this report, has made it possible to secure with comparatively small instruments photographs of nebulæ greatly surpassing in delicacy of detail the best results previously known. This new era was inaugurated by the late Director Keeler of the Lick Observatory, whose discovery that the typical nebula is spiral in form, and that tens of thousands of these spirals are scattered over the heavens, is of fundamental importance in connection with the great problem of stellar evolution. In spite of the undoubted evidence of motion revealed in the very forms of these nebulæ, no actual change in their outlines has as yet been detected. It is very important that an extensive photographic campaign should be undertaken with a very powerful telescope, for the purpose of securing a permanent record of these nebulæ as they exist today. For comparison with photographs taken fifty or one hundred years hence such a series of photographs would be invaluable.

Photography of Moon and Planets.—Reference has already been made to the photographs of the Moon obtained on wet collodion plates by Rutherfurd. For many years no advance beyond these remarkable results was made, in spite of the advantages arising from the use of gelatine dry plates. It remained for the then recently erected 36-inch refractor of the Lick Observatory to give still sharper photographs in 1890. About the same time, in 1889, similar results were obtained by the Harvard astronomers, enlarging the image in the telescope. The most extensive series of lunar photographs hitherto obtained is that at the Paris Observatory, where Loewy and

Puiseux have used the great equatorial coudé for this purpose with striking success. Their great photographic atlas of the Moon, with the atlas prepared by Weinek from Lick and Paris negatives, provide much material for a study of lunar topography. The recent work of Ritchey with the 40-inch Yerkes telescope, a visual instrument adapted for photography by the use of a yellow color screen, has yielded remarkable photographs of the Moon, which should be of service in the study of the smaller details. The smallest markings perceived under good conditions at the telescope are yet to be recorded photographically, but in spite of this fact the best photographs now available are much superior to visual observations for some classes of lunar research.

Photographs of planets which are at all comparable with visual images have not yet been obtained, though the results of Common, the MM. Henry, Schaeberle, W. H. Pickering, and others give reason to hope that marked advances may be expected in the future. Judging from the results of recent experience, it is probable that the improvement of both lunar and planetary photographs will come through the use of larger focal images. An increase in the sensitiveness of photographic plates, unless it involved a corresponding increase in the size of the silver grains, would be of the greatest service in this and in most other departments of celestial photography. For this and other reasons the encouragement of research in photography, which might lead to the improvement of sensitive plates, is greatly to be desired.

Respectfully submitted.

E. C. Pickering.

REPORT OF ADVISORY COMMITTEE ON PALEONTOLOGY

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: Your Advisory Committee on Paleontology would respectfully report as follows:

The principles upon which research in paleontology should be encouraged appear to be very similar to those in zoölogy. The problems and methods are different. It is not necessary, for example, to institute a permanent paleontological station.

First, it is very desirable to *encourage certain explorations*, especially such as are not provided for by any existing government or state institutions.

Second, the committee is very strongly of the opinion that a *more exact correlation of European and American horizons*, or the establishment of standard bench-marks for the geological time scale, is a matter of first importance as a problem. This is expanded under IV.

Third, a publication of the same character as the *Paleontographica* of Germany, or the memoirs of the Paleontological Society, is one of the greatest needs of American paleontology at present, as there are no institutions with funds for the adequate preparation of illustrations. A number of valuable memoirs have already been offered to the Institution.

Fourth, awards or special grants for paleontological research are desirable on the principles carefully considered and hereinafter expressed in sections I and V.

More in detail our recommendations are as follows:

I. *General principles.*—That a comprehensive plan for the encouragement of paleontological research extending over a number of years should be prepared, and that no special applications should be considered or acted on until certain general principles are established. Chief among these are (*a*) that it is desirable to aid existing institutions, not to duplicate their work, either in research or publication, but to strengthen and coöperate; (*b*) that diffusion of the advantages of the Institution throughout the existing centers of research will effect better results than centralization.

II. *Advisory committee.*—That, as a feature of the permanent plan of the Institution, an advisory committee of five on paleontology

be constituted on the rotation system, to represent as far as possible—

Vertebrate paleontology—fishes and amphibians.

Vertebrate paleontology—Birds, reptiles, and mammals

Invertebrate paleontology—Paleozoic

Invertebrate paleontology—Mesozoic and Cenozoic.

Paleobotany

This committee to be chosen for terms of five years, but at the first election to determine by lot the members to hold office for one, two three, four, and five years respectively, so that one member of the committee shall go out of office each year and not be eligible for immediate reëlection, but to be replaced by a new member elected by the Carnegie Institution.

Such advisory committee to hold stated quarterly meetings, for attendance at which members present will receive a fee in addition to expenses, to elect its own chairman annually, and to appoint annually a salaried secretary, whose duty it shall be to keep all the records, to prepare and present applications, and to act especially as editor of paleontological publications.*

The principle of rotation in office, both as regards the committee of five and its secretary, will secure the constant infusion of fresh blood and keep the committee abreast of the most recent advances in paleontology.

The organization, duties, and powers of this committee will naturally conform to those assigned by the Institution to advisory committees on other subjects.

III. *Personnel.*—That a full list of the active investigators of the country, including the younger as well as the older and better known men, should be prepared and kept constantly renewed, in connection with the specific problems in regard to which investigation from time to time seems to be most urgent.

IV. *The establishment of standard bench-marks for the geological time scale.*—It is recommended that the Advisory Committee on Paleontology be authorized (and in case no such committee is appointed, that a committee be appointed for this purpose) to undertake the establishment of a few carefully selected time bench-marks, which shall constitute a permanent set of standards for discriminating divisions of geologic time, and which shall serve as datum

*It is probable that the salaried secretary could also act as the secretary of one or more other committees, say the zoölogical and be connected with the central office in Washington.

planes for the correlation of geological formations and events of other continents than those in which the standards are established.

The imperfection of the standards now in use arises from the fact that they were originally defined and established in pre-evolution times, before the reality of the gradual and continuous modification of the life of the globe was appreciated, and only the rocks between indefinite boundaries are defined by lists of their characteristic species, whereas it is now seen to be essential to determine the precise stage of evolution of the races whose representatives lived before as well as after any artificial time boundary which may be adopted as a standard

The sections which must be used in establishing such time bench-marks are chiefly in Europe, and because they are within easy reach for personal examination European geologists may not appreciate as do foreigners the necessity for their precise and full definition.

American geologists have been driven to appreciate the importance of these standard time bench-marks for the discussion and classification of geological problems, and to realize that accuracy in all discriminations of geological time values in America and throughout the world depends upon the establishment of a single set of standards with which to make accurate correlations of secondary standards in other continents.

The task of establishing such standards will involve comparative and international investigation, and it is therefore outside the legitimate field of the government Geological Survey of any particular country. It will involve also an amount of travel and work in countries foreign to the investigator's home which places it beyond the capacity of private enterprise of such paleontologists as would alone be competent to do the work.

It is probable that coöperation with foreign geologists, and perhaps with official state surveys, may be required in executing the task, and it is suggested that the *Congrés Geologique Internationale* is a convenient agency through which such coöperation may be attained.

Considerable investigation, correspondence, and possibly travel will be required before it can be definitely estimated what shall be the extent of the investigation and the amount of expense necessary to complete it

It is therefore recommended that for the first year the Carnegie Institution provide the means for a small committee of experts, authorizing them to prepare a plan for carrying out the investigation herein described, but leaving the decision as to the amount of ap-

propriation to be allotted to the execution of the work until the committee shall make further report and recommendation.

V. *Awards of grants.*—That, with certain exceptions, it is desirable, instead of permanently detailing the investigators of the country to certain subjects, possibly to the detriment of the institutions with which they are connected, to establish quarterly, half-yearly, or yearly grants. These grants should be apportioned according to the importance of the subject or the ability of the investigator, and should be at least equivalent to the salaries for the same period received by the investigators from the institutions with which they are connected. In this manner teachers, curators, state and government scientists would be entirely relieved of routine work for certain periods, and would be enabled to devote their time exclusively to certain investigations, At the same time these institutions would be able to provide substitutes and the work in the various laboratories would not be interrupted, but materially benefited and enriched. The committee feels very strongly at the present time that the chief drawback to progress in discovery in paleontology is that the majority of the ablest men are so heavily burdened with administrative work that they do not enjoy the repose and opportunity necessary to research. At the same time the students and younger generation of the country would suffer if the ablest men were permanently withdrawn by the Institution.

VI. *Exploration.*—Paleontological exploration in general in this country is well provided for by the museums of New York, Chicago, Pittsburg, New Haven, Kansas, and the United States Geological Survey, among other institutions. More distant exploration, such, for example, as the Antarctic expedition proposed by Mr. J. B. Hatcher, should be supported on the side of paleontology as well as zoölogy, because likely to produce very important results.

VII. *Publication.*—In addition to the publication of such paleontological memoirs as are not provided for by existing agencies, we recommend the publication of a Journal giving brief statements and short summaries of the results of all paleontological investigations—a sort of "Comptes Rendus."

Respectfully submitted.

HENRY FAIRFIELD OSBORN, *Chairman*,
HENRY SHALER WILLIAMS,
Committee.

NEW YORK, *October 1, 1902.*

REPORT OF ADVISORY COMMITTEE ON ZOOLOGY

CONTENTS.

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: The undersigned members of the Advisory Committee on Zoölogy respectfully present the following report. Professor Alexander Agassiz was originally a member of the committee, and the report embodies much of his valued advice. We greatly regret that other duties prevented his remaining on the committee until the completion of the report. Dr. C. Hart Merriam was prevented from attending the final meeting, and some of his views, expressed by letter, are inserted at the close.

Our recommendations are as follows:

I. GENERAL PRINCIPLES.

That a comprehensive plan for zoölogical research extending over a number of years should be prepared, and that no special application should be considered or acted upon until certain general principles are established. Chief among these are the following:

1. That it is desirable to aid other existing institutions; not to duplicate their work, either in research or publication, but to strengthen and coöperate. This is further explained under V.

2. That it is also desirable to promote certain independent explorations, expeditions, researches, and publications in the name of the Institution. This is further explained under V and VII.

3. That, with certain exceptions hereinafter mentioned, diffusion of the advantages of the Institution throughout the existing centers of research will on the whole effect better results than centralization.

II. Permanent Advisory Committee.

That as a feature of the permanent plan of the Institution, an advisory committee on zoölogy somewhat similar to this be constituted on the rotation system, to act as advisers in connection with the Marine Biological Laboratory and Experimental Station, the encouragement of research, expeditions, and all applications for grants, etc., in the field of zoölogy.

More in detail, that this advisory committee of five be chosen to represent the following subjects.

1. The zoölogy of vertebrates.
2. The zoölogy of invertebrates.
3. Embryology and cytology.
4. Experimental morphology.
5. Biology, œcology, evolution, heredity, biometrics, geographical distribution, etc.

This committee to be chosen for terms of five years, but at the first election to determine by lot the members to hold office for one, two, three, four, and five years, respectively, so that one member of the committee shall go out of office each year, and not be eligible for immediate reëlection, to be replaced by another member elected by the Carnegie Institution. For this purpose the committee shall submit annually a list from which the Institution may select the new member. In this manner the committee will keep thoroughly informed as to the past and will be freshened constantly by the accession of a new member each year.

Such advisory committee to hold stated quarterly meetings, for attendance at which members present will receive an adequate fee in addition to expenses.

Such committee to elect its own chairman annually.

Such committee to have the power given by the Institution to appoint annually a salaried secretary, preferably a scientific man, whose duty it shall be to keep all the records, to prepare and present applications, and to act especially as business editor of zoölogical publications.

The organization, duties, and powers of this committee will naturally conform to the rules adopted by the Institution for advisory committees on other subjects.

III. Personnel.

We recommend that a full list of the active investigators of the country, including the younger as well as the older and better known men, be prepared and kept constantly renewed, in connection with the specific problems in regard to which investigation from time to time seems to be most urgent.

IV. Marine and Experimental Stations.

While, as stated above, coöperation should be the main feature of the Institution, the committee strongly indorse the establishment of a permanent biological laboratory as a central station for marine biology in general, with branches at such other points as may seem desirable; also affiliated or independent experimental stations for the study of physiological zoölogy and problems relating to heredity, evolution, etc.

Since the publication of researches from the marine and experimental stations in the future will be so closely related to zoölogy in general, it would appear advisable to refer this general question to the permanent advisory committee on zoölogy.

As regards an experimental station, among the most important desiderata at present are experiments in heredity, in variation, in instinct, in modification, all of which should extend over a series of years and be planned systematically.

We would advise referring report on this matter to a committee of specialists, who are best able to present a permanent plan. Naturally consideration will have to be given first to the question whether the best results will be achieved by making this station a part of the marine station or placing it under separate direction and in another locality. Our main purpose now is strongly to recommend the establishment and endowment of such a station or stations.

V. Expeditions and Explorations.

As stated in the brief digest of applications received,* the whole scheme of exploration by the Carnegie Institution is admirably set forth, especially by Stejneger, Mayer, and Hatcher. All are of great interest and importance. These explorations involve very large sums of money and expenditures extending over many years.

* Here omitted.

VI. Special Grants.

With certain exceptions, it is desirable, instead of permanently detailing the investigators of the country to certain subjects, possibly to the detriment of the institutions with which they are connected, to establish quarterly, half-yearly, or yearly grants. These grants should be apportioned according to the importance of the subject or ability of the investigator, and should be at least equivalent to the salaries for the same period received by the investigators from the institutions with which they are connected. In this manner teachers, curators, state and government scientists would be entirely relieved of routine work for certain periods, and would be enabled to devote their time exclusively to certain investigations. At the same time these institutions would be able to provide substitutes. The work in the various laboratories would not be interrupted, therefore, but materially benefited and enriched. The committee feel very strongly at the present time that the chief drawback to progress in zoölogy is that the majority of the ablest men are so heavily burdened with administrative work that they have no opportunity for continuous research; nor are they able to command the means for carrying on zoölogical researches at remote points. At the same time the students and younger generation of the country would suffer if the ablest men were permanently withdrawn from instruction by the Institution.

Exceptions will naturally arise in case of unattached investigators or of those detailed by the Institution for prolonged expeditions or researches.

As regards grants or subsidies for research, provision should naturally be made for visiting museums both at home and abroad, for the examination of collections, for supplying investigators with necessary books and apparatus from a central bureau, and with the means for carrying on special researches in the localities where the opportunities are to be found.

Investigators should enjoy considerable freedom of action. It frequently happens that while in the field unforeseen opportunities arise for new lines of work, and the most effective investigator is the one who knows how to seize such opportunities and make the most of them.

VII. Publication.

As regards publication, it is desirable to establish a central publication bureau of the Institution to regulate the issue of large *monographs in quarto* form and of *shorter papers in octavo* form. As suggested above, the salaried secretary of the Zoölogical Committee could also act as editor.

Zoölogical monographs treated from the anatomical, embryological, and biological standpoint, similar to the monographs issued by the Naples Station, are of great importance. The committee also recommend the preparation and publication of systematic monographs of groups in zoölogy, studies of faunal areas and comprehensive studies of the geographic distribution of life.

Without recommending in detail, it may be said that fully one-half of the applications for special grants for research and for publication are worthy of very careful consideration. They suggest work of just the character which the Institution, it appears, is especially designed to undertake and encourage.

The urgent character of these requests indicates that sufficient means are not yet provided in this country for publications of importance. At the same time, the committee are of the opinion that several of the above applications, together with a number of minor applications included in the general list, might more properly be provided for by the United States Fish Commission, the United States National Museum, the Boston Society of Natural History, the National Academy of Sciences, and other institutions.

In connection with the matter of publication, reference may be made to a letter dated April 28 from E. B. Wilson to Professor Walcott, in which a general scheme for zoölogical publication is outlined and discussed at some length.

VIII. Subsidies.

Cases may arise, such as that suggested by the editor of a well-known journal, where a subsidy might wisely be extended to a journal of great value, "the income from which from subscriptions, sales, etc., amounts to less than half the cost of publication, the balance having been made up by contributions from the editors and other outside sources." This, again, is a matter where some general principle of action must be adopted by the Institution before a recommendation can be made.

The Zoölogical Station at Naples will in all probability be one of the most important centers for special research work connected with the award of special grants mentioned under VI. It is therefore desirable, and this committee strongly recommend, that the Carnegie Institution subscribe annually for a table at Naples to the value of five hundred dollars ($500). Among the applications which will presently be received is one for a special line of investigation at this station.

Respectfully submitted.

HENRY FAIRFIELD OSBORN, *Chairman*,
EDMUND B. WILSON,
W. K. BROOKS,
Committee.

NEW YORK, *October 15, 1902*.

IX. SUPPLEMENTARY NOTES.

Extract from letter of Dr. C. Hart Merriam, dated June 7, 1902.

"While in accord with much of the report, I find myself more and more opposed to the plan of scattering the work and funds of the Institution. I am opposed, therefore, to the first of the general principles stated. It seems to me that, as a rule, existing institutions should be allowed to continue their work without aid or interference from the Carnegie Institution.

"Its affairs should therefore be conducted from the first with an appreciative sense of the dignity, unity of purpose, and continuity of effort essential in an organization which means so much for human progress. Its strength and influence should not be weakened by diluting and scattering its resources, but husbanded for uses in keeping with the promise and scope of the Institution. It is quite conceivable that its rich endowment might be so distributed as to partake of the nature of charities to individuals and institutions.

"For some time I shared the views expressed in the committee's report, but have gradually come to change my position in this matter. I am now fully convinced that the Carnegie Institution should carry on its own work, under its own name, and should publish the results in its own series of publications."

Note by E. B. Wilson.

I am of opinion that the regular support by the Carnegie Institution of at least two tables at Naples is highly desirable. The advantages derived by American biology as a whole from the Naples station in the past have been of incalculable value ; there is every reason to look forward to at least equal benefits in the future if sufficient opportunity be given.

There are at present three American tables at Naples—one supported by American women, for the use of women; one by the Smithsonian Institution, and one by private subscription. Both the latter are of uncertain tenure. Although payments for the last named are in arrears, Professor Dohrn has generously continued the table despite the existence of a considerable deficit for past years.

There are for the current twelve months more applicants than can be accommodated by all three tables combined.

Note by Henry F. Osborn.

The Concilium Bibliographicum at Zurich, under the direction of Dr. Herbert Haviland Field, an American, is worthy of an annual subsidy of not less than $500 for its catalogue and digest of all current zoölogical literature, invaluable to workers in every branch of zoölogy.

REPORT OF ADVISORY COMMITTEE ON PHYSIOLOGY

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: During the last thirty years the greatest advances in our knowledge of disease have been made through bacteriology, *i. e.*, through the study of the life history of microbes. But it has now been shown that the microbes are injurious not by their simple presence in the body, but through the chemical products of their activity, and that antidotes for the disturbances thus produced are to be found in chemical substances formed in the animal body as the result of reactions set up in the tissues and fluids by the microbes or their toxic products. The study of these products and their antidotes must of course be prosecuted by the methods of physiological chemistry, and it is, therefore, by the aid of this science that we may expect the next great steps to be taken in the advancement of medical science. But in order that these steps may be firmly planted and lead to an accurate knowledge of those disturbances of function which constitute disease it is essential that the normal metabolism of the body should be much more thoroughly understood. Here, too, it is largely by means of physiological chemistry that advances must be made, and it is thus evident that it is more largely for the chemical than for the physical side of physiology that, in the immediate future, special provision for coöperative work should be made.

The most important problems of physiological chemistry are, perhaps, those which are connected with nutrition, and there is little doubt that the establishment of a well endowed laboratory, provided with all the apparatus needed for the study of the nutritive phenomena of the animal body, would in a few years lead to results of the greatest importance for the welfare of humanity. Such a laboratory would, however, have to be provided with certain forms of physical apparatus, for the study of the physical phenomena connected with nutrition should go hand in hand with that of the chemical processes.

A laboratory thus established and equipped would be something quite different from any existing institution, for though certain laboratories possess particular forms of apparatus adapted to researches of this sort, there is no place where the student of nutrition (in the broad sense of the word) can find under one roof all the special

appliances which he may need to employ in his researches. The question of the proper location of such an institution need not be discussed until the Board of Trustees has decided whether the establishment and equipment of laboratories is to be regarded as a part of the recognized work of the Carnegie Institution.

Another way in which it is desirable for the Carnegie Institution to aid in the advancement of physiological science is by assisting individuals of proved ability in their researches. This may be done—

(1) By supplying them with apparatus and material for study which they could not otherwise obtain.

(2) By affording them assistance, either clerical, laboratory, or editorial, thus relieving them of the drudgery of routine work and enabling them to expend their energies in directions in which they will be most effective.

(3) By making it possible for them to secure leave of absence from their work as teachers in order to devote themselves for a year or more to some special research, either in a laboratory already established or in the laboratory for the study of nutrition, the creation of which has been above suggested.

Your committee is of opinion that the demands made by modern methods of education in physiology and by the increase in the size of classes has caused some competent observers to give up all attempt at original work. For like reasons, men who enter laboratories as demonstrators and assistants are soon overwhelmed by the necessity of giving up all their working hours to instruction. Whatever is done to aid physiological research must insure to the worker full relief from the labor of educating students.

In the case of tried men, now too actively employed as professors, it will be advisable, when they desire to conduct researches of sufficient importance, either to secure easing of their work by assistance or to enable them for long enough periods to be free from all need to teach.

Respectfully submitted.

S. WEIR MITCHELL, *Chairman*,
H. P. BOWDITCH,
W. H. HOWELL,
Committee.

OCTOBER 16, 1902.

REPORT OF ADVISORY COMMITTEE ON ANTHROPOLOGY

CONTENTS.

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN. Your Advisory Committee on Anthropology has the honor to present the following report, which can not be regarded as more than preliminary, owing to the wide extent of the subjects to be considered, to various questions that naturally arise, such as those of infringement on territory already occupied, as well as to the difficulty of finding men specially qualified for certain divisions of research.

I. PAPERS REFERRED TO THE COMMITTEE.

The correspondence referred to the committee has been considered and brief notes characterizing the subject matter are attached to the communications in each case. Many of the papers are found worthy of careful attention, while a number relate to subjects not within the scope of the Advisory Committee on Anthropology. No grants to individuals or for special. limited researches are recommended, but rather it has been sought to organize the work on broad lines, as a basis for future elaboration in special lines. However, many suggestions made by correspondents have been here embodied.

II. THE FIELD OF ANTHROPOLOGY AS A WHOLE.

The science of man covers a wide range of diversified subjects, which may be classified under a few general heads. Viewing the human kind or species as a whole, the science considers (1) its physical or biological characters under the head of *physical anthropology;* (2) its intellectual characters and history under *psychology;* (3) its arts and industries under *technology;* (4) its social structures and functions under *sociology;* (5) its languages and letters under *philology;* (6) its systems or opinion and its cults under *philosophy and religion;* (7) its æsthetical activities under *æsthetics.*

The study of these phenomena is carried on with two principal ends in view, viz., (*a*) to write the history of the race, (*b*) to discover the principles and laws of human development with the view of utilizing them to regulate the present and to mould the future of the race. As yet, these researches are hardly begun, because of the vastness of the subject, the inadequacy of financial support, and the limited number of students engaged in the work.

It is to be observed that a number of branches of the general subject have been highly specialized, and that some are, on this account, regarded as independent fields of research. Such are social science, political economy, and psychology. These branches your committee have considered only so far as every research that has to do with peoples and cultures must deal with all phases of the science of man. The special requirements of these branches have not been weighed, and no recommendations are made with respect to them. Essential coördination of the several branches can be considered when the reports of the various committees are compared.

Although, as above indicated, researches have been initiated in many parts of the anthropological domain, of none of the fields is the occupation complete, and in no part is it so vigorous as to satisfy the requirements of science. In this connection it may be further mentioned that the urgency of the demand for research is due not a little to the fact that primitive culture, in which are hidden the keys of history, is being rapidly destroyed by the spread of civilization, and that the peoples of the earth are fast losing their original race characteristics.

III. Present Researches in Anthropology.

American students have naturally and properly turned their attention mainly toward researches among the aborigines, and it seems advisable to continue for the present along the old lines.

The more important agencies at present actively engaged in anthropological researches may be briefly noted:

1. The Bureau of American Ethnology, established in 1879 for the investigation of the North American Indians, has latterly extended its field to cover the American continents and islands, so far as the law and its means permit. Its investigations refer particularly to the languages, social customs, beliefs, arts, and industries of the aborigines, especial attention being given to the tribes of the United States.

2. The United States National Museum has been conducting researches throughout the world, based chiefly on the material in the Museum, though partly on field study and collecting, special stress being laid on technology.

3. The Peabody Museum of American Archæology and Ethnology at Cambridge has devoted its energies to the investigation of the archæology of the United States, Mexico, and Central America.

4. The American Museum of Natural History, in New York, prosecutes work in ethnology, archæology, and physical anthropology, especially in North America, in Mexico, and in northeastern Asia.

5. The Field Columbian Museum, in Chicago, is making large collections and conducts ethnological and archæological researches in western-central North America.

6. The Free Museum of Science and Art, in Philadelphia, is bringing together collections from various parts of North America and subjecting them to critical study.

7. The Ohio State University maintains a department of archæology and a museum devoted mainly to local studies and collections.

8. The University of California is making investigations in archæology and ethnology, chiefly in California.

9. The National Museum of Mexico is accumulating a rich collection and conducting investigations in archæology and, to some extent, in the ethnology of the Republic.

10. Collections and investigations relating to the archæology of Canada are made, and a museum is maintained in Toronto in connection with the Department of Education.

11. The British Association for the Advancement of Science has carried on archæological researches in Canada since 1888.

Several institutions in South American countries are engaged in related work. The Archæological Institute of America, although mainly engaged in researches in classical and oriental archæology, has recently taken up America. Several American universities and colleges devote more or less attention to local or general archæology and ethnology. Investigations in archæology are pursued at the Harvard and Columbia Universities and at the University of Chicago.

American anthropology finds active patronage in several voluntary associations. The Section of Anthropology of the American Association for the Advancement of Science has long stood at the head of such organizations. The American Anthropological Association has recently been founded and already embraces in its membership all the leading anthropologists of the country. The Anthropological

Society of Washington and the American Ethnological Society of New York are doing excellent service, both local and general, in various branches of anthropology.

The greater part of research, with the exception of that of the Bureau of American Ethnology, that of the voluntary societies, and that of the universities, is characterized by its immediate relation to the material culture of the aboriginal tribes, and very little investigation is being carried on by these institutions that can not be illustrated by museum specimens. For this reason the anthropological work done in America is somewhat one sided, the physical branch especially remaining undeveloped.

It may be appropriately noted in this connection that the concentration of American anthropology upon the peoples of our continent has the effect that the general comparative points of view are in danger of being obscured by the excessive weight given to local phenomena. It is one of the greatest desiderata for the training of the young ethnologists that they should be given opportunity to become acquainted with the culture of Africa, Asia, and of the islands of the Pacific ocean.

The following suggestions are made with the view of strengthening what seem to be the weaker lines in American anthropology, thereby rounding out and perfecting the science as pursued in this country.

IV. Suggestions as to Researches in Anthropology by the Carnegie Institution.

(1) *Researches in Physical Anthropology.*—A most important field of research comprises the problems of physical anthropology. Facilities furnished by present institutions are entirely inadequate, and the investigation has at no point been systematically pursued. These vastly important problems can be successfully solved only in a well-equipped laboratory, the permanence of which for a considerable time is assured. One of the most important problems to be treated is the development of types of mankind from childhood to the adult stage as determined by heredity and environment. Up to this time it has never been possible to cultivate, systematically, this important field of investigation, for lack of means and men. It is therefore suggested to establish a central anthropometric and psychometric laboratory, which shall collect and discuss data on the development of physical types.

The treatment of the problems should be based on continued observations upon the same individuals from early childhood until maturity and old age. The characteristic physical and mental development of various races and types of men can be determined by this means. The collection and discussion of this material will throw light upon the laws of heredity, upon the development of types, and upon the conditions which favor and retard physical and mental development. The last named subject is one which previous results have shown to be of great practical importance.

(2) *Researches in Archæology.*—In America the anthropologist has to deal with various classes of aboriginal remains which illustrate the pre-Columbian history of the native peoples. Two somewhat distinct divisions of research are included. The first relates to the better known remains of cities, towns, dwelling sites, fixed works, and artifacts of many classes: the second to such of the earlier remains of man and his handiwork as bear definite relations to geological and artificially stratified formations, thus affording a basis for chronologic differentiations Within the area of the United States the former division has received much attention, and numerous agencies are now engaged in its study, so that no additional work is recommended. In middle and South America much is still to be done, but your committee has only been able to find, and that at the last moment, one person properly qualified for this branch of research.

The second division of this subject may well receive attention from the Carnegie Institution. The phenomena are scattered and obscure, and agencies now in existence have not been able to enter systematically upon their study. The geological formations of both continents, ranging from Eocene to Recent, abound in varied records, but investigation has been, in the main, desultory and unscientific, and the isolated observations are today without adequate correlation.

If researches in this field be undertaken, the first step should be a compilation of all available data and a correlation of the results of previous investigations. The field work should begin preferably near home, extending later to favorable localities in various parts of the world. Examinations should extend to deposits in rock shelters, caves, and caverns, where men have lived and where horizons are so marked as to afford a basis for chronology. They should include various other ancient occupied sites, such as kitchenmiddens, shellheaps, and earthworks, whose strata serve to indicate successive occupations. Researches in glacial formations where traces of man

occur are of the utmost importance, and outside of the glacial areas there are many sites that, properly studied, should yield most valuable results.

It is recommended that researches in this field be taken up, beginning with the exploration of rock shelters and caves in the eastern United States. The aid of competent geologists should be sought in selecting such as are of considerable antiquity, and in which the deposits are likely to afford definite indications of chronologic sequence.

(3) *Researches in Ethnology.*—The third field of research to which special attention is called is the systematic study of the fast vanishing tribes of American aborigines. As before indicated, it is receiving limited attention from various agencies which are, however, inadequate to the needs of the case. It is therefore suggested that the Carnegie Institution take up systematic studies in this field.

In different parts of America distinct forms of aboriginal culture have developed. The importance of carrying on investigations concerning these with the greatest energy and with as little delay as possible can not be too emphatically urged. In the area of the United States and Canada alone, approximately 350 languages are spoken. Of these languages, not more than 20 are tolerably well known, while of the rest we have nothing but brief vocabularies and unsatisfactory grammars. Each tribe that speaks a language of its own has also a culture of its own that should be investigated. A comparison of languages and cultures in detail is the only means of reconstructing the earliest history of the American race, and only the reconstruction of this history can give us the comparative material that should be furnished by our continent toward the study of the laws of development of human culture. Inside of the next ten years one third of the remaining 350 languages will have disappeared. In twenty five years it will be impossible to obtain of these languages more than vocabularies, and the culture and native ideas will have disappeared completely. These statements apply with almost equal force to the native peoples of Mexico, Central and South America. If this great body of the subject matter of human history is to be saved for the future, active researches should begin at once. Up to this time only a few tribes have been studied with any degree of thoroughness. Existing agencies are investigating throughout the following tribes:

Eskimo,	Sioux,
Tribes of the Northwest Coast,	Arapaho,

Salish,
Northern California,
Iroquois,
Kiowa,
Pueblos,
Shoshones.

Superficial or fragmentary work has been done among the following tribes:

Pawnee,
Lower Mississippi,
Muscogee,
South California,
West Algonquin (except Arapaho),
Athapascan,
Kutenay,
And other small tribes.

All this work is urgent. At the present time the various tribes of the Caddoan stock deserve special attention. They have never been studied scientifically, and today they are rapidly decreasing in numbers, the total population being about two thousand two hundred. To this stock belong the Pawnee, the Arikara, the Wichita, the Caddo, and the Kichai. Taken collectively they form the least known and most interesting large group of Indians in the United States. Apart from the intrinsic merits of these tribes as subjects of investigation, they occupy and have occupied a median position between the tribes of the Great Plains and those of the extreme south, and hence form a most interesting connecting link. All the tribes of the group have an extensive mythology and very elaborate ceremonies, with rituals which are chanted and which for their value in the study of primitive culture are unparalleled. Their material culture was comparatively high, and, among other things, they evolved two highly specialized forms of habitation, the Pawnee earth lodge and the Wichita grass lodge, in the construction of which most interesting ceremonies are performed. Because of the complexity and multiplicity of their ceremonials, and especially on account of the necessary expense involved, no investigator has hitherto devoted himself exclusively to this group. The expense of ordinary work of this nature is here more than doubled, because the many hundreds of songs must be recorded on a phonograph and transcribed in both words and music, and to secure this material requires an exceptional interpreter, for all these rites are secret, and the interpreter must not only be influential, but must be of the priesthood. Again, all the rituals are regarded as personal property, and may not be transferred except on payment of goods or money. In addition to the many ceremonies with elaborate rites, the Pawnee have also sacred and secret bundles, which are only opened ceremonially when an

extended ritual is sung There are over four hundred and fifty songs in a single bundle. One of the bundles is dedicated to the morning star, and formerly was opened only when the rite of human sacrifice was performed.

The object of the suggested research, then, is to complete a study of the culture of the different tribes of the Caddoan stock, special attention being given to (1) the rituals of the Pawnee ceremonies and sacred bundles, and (2) the mythology and linguistics of all four tribes.

Respectfully submitted.

W. H. HOLMES, *Chairman*,
FRANZ BOAS,
GEORGE A. DORSEY,
Committee.

OCTOBER 16, 1902.

REPORT OF ADVISORY COMMITTEE ON BIBLIOGRAPHY

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: Your Committee have thought that the Trustees might wish to have the consideration of particular projects prefaced by a survey of the general field in each department of science with which it may propose to deal. In this belief the following statement is presented:

In the case of Bibliography a concise statement is impracticable, not merely because the field is vast and indefinite (embracing as it does not one science, but a consideration of the literature of *all* the sciences), but because the work already done, or in progress, or projected, is so considerable and includes undertakings so numerous and so diverse in scope and method that the precise area covered by them can not briefly be described with precision.

A mere catalogue of existing bibliographies would, it is estimated, comprise over 25,000 entries.*

The appended memorandum,† drafted by Dr. J. D Thompson and other bibliographers of the staff of the Library of Congress, indicates certain of the more notable achievements or projects to date. It is appended, not as a complete statement of the work already done, nor as a demonstration of the work which remains to be done, but as a suggestion of the multitude and diversity of the undertakings which must be examined before the opportunities remaining to the Carnegie Institution can be fully defined.

A brief reference to the memorandum will indicate the extraordinary activity that has existed and still exists in bibliographic research and publication. There have been bibliographies covering certain departments of literature, or certain periods, or certain geographical areas; the literature of the past or the literature in process of issue; and even attempts (of which one is still in progress) to cover all the existing literature on all subjects The work has been done in part as a commercial venture, in part by societies, institutions, or governments, as a contribution to knowledge. To bibliographies, properly so called, which attempt to exhibit all the

*Margerie's "Catalogue des bibliographies géologiques" alone contains nearly 4,000 entries.

† Not here printed.

literature upon a given subject, or within a certain area or period, there must be added the catalogues and topical lists issued by libraries of material in their own collections, where the collection has been developed with a view to relative completeness. Nor can there be omitted from consideration trade catalogues, reviews in current journals, and selected lists of authorities appended to treatises; for any proposal for a grant by the Carnegie Institution assumes that the investigator is in present need of information as to the literature of his subject not now conveniently, precisely, or adequately accessible to him. If, for example, it be proposed for the Institution that it shall undertake a comprehensive bibliography, the work of the Institute at Brussels must be reckoned with; if a national bibliography for the United States, Sabin, the American Catalogue, the publications of the American Library Association, the Card Indexes of the Library of Congress, and other undertakings which in the aggregate are likely to cover, even though unevenly, this area; if the literature of the natural and physical sciences, the Royal Society Index, the International Catalogue of Scientific Literature, the publications of the Concilium Bibliographicum at Zurich, and the various Centralblätter and Jahresberichte, etc., etc.

On the other hand, the existing bibliographies vary greatly in form, in method, in accuracy, in completeness, and in accessibility; so that the mere existence of a bibliography dealing with a certain branch of science, or a certain period or area, is by no means conclusive against a proposal for further work within the same field—or in continuation—for the subject matter continuing, the bibliographic record of it is never ended. A bibliography thorough within its field may be insufficient because it includes entries by author only, while the investigator requires a classification by subject; or it may be on cards, while his convenience requires a publication in book form; or, if of current literature, its issue may be so tardy as to defeat its utility; or, having all merits to his need, it may have failed, or be in peril of failing, as a commercial venture and require and justify a grant in its aid.

Your committee deems it futile in this report to define a resultant appropriate field of activity for the Carnegie Institution. It contents itself with responding to the particular proposals already submitted, which it has endeavored to test by a consideration of the existing facilities, and to recommend for immediate action only two, the first of which (the Index Medicus) has already demonstrated its utility and necessity, and the second of which (the Handbook to

Learned Societies) is not so much a bibliography as a necessary preliminary to *any* thorough bibliographic work involving the literature of science.

Respectfully submitted.

HERBERT PUTNAM, *Chairman*,
CYRUS ADLER,
J. S. BILLINGS.
Committee.

OCTOBER 20, 1902.

Supplementary Suggestions by the Chairman of the Committee, January 5, 1903.

The field of bibliography is all existing literature, with continuations.

From time to time there have been projects of a universal bibliography. There is one such project now under way. The International Institute at Brussels is attempting a universal catalogue, by author and by subject, or rather by class, the classes being based on the decimal system. The entries are, for the most part, composed of clippings from catalogues. They are thus made at second hand. They lack the bibliographic value which exists in an entry made direct from the book itself. They are on cards, and thus lack the utility possible in a catalogue, copies of which are multiplied in book form for distribution. Granting, however, the possibility of a universal bibliography, no member of your committee would, I think, have recommended it for the consideration of the Carnegie Institution. The field is too vast, the expense too great, the utility of the results, in the only form which they could be secured, too doubtful.

The field of bibliography may be *divided* in three ways: (*a*) by territory, (*b*) by subject, (*c*) by period.

(*a*) In a division by territory the area for the Carnegie Institution would naturally be the United States. Attempts to cover the literature of this country are indicated under National Bibliographies, United States, on page 5 of the Appendix to the Committee's Report. The completion of Sabin, which now comes down to the letter S, is highly desirable, but it is likely to be undertaken as a commercial venture by the successors of the firm which instituted the work. The printed cards of the Library of Congress will in the course of

the next five years embrace the largest single collection of American publications, in the National Library, which is attempting to secure every American imprint of possible concern to research. The printed cards of Harvard University, the Boston Public, and the New York Public libraries (copies of which will be on deposit at the National Library) will in large measure supplement the record based upon its own collections.

In view of these and other sources of information open to the serious investigator, the Carnegie Institution could not, I think, be asked to undertake a national bibliography for the United States.

(*b*) Division by subject: The area appropriate to the Carnegie Institution will, of course, be that with which the research may be concerned which it is its intention to promote. This is, Science. How far this term extends has not, I believe, as yet been defined. The assumption has been general that preference would be given to the natural and physical sciences. In these the material of most concern to the investigator consists (1) of the current publications, and (2) of the publications of the preceding ten years, or at least of the preceding quarter of a century.

Current publications are to be covered by the International Catalogue of Scientific Literature. This catalogue will be based upon contributions from twenty-seven regional bureaus. Were not the Smithsonian already the bureau for the United States, the Carnegie Institution might well become so. No contribution by it to bibliography in aid of research could be more appropriate or more useful than this: the territory, the United States; the subject matter, the natural and physical sciences; the period, the present and the future.

Certain sciences are not to be included in the scope of the International Catalogue. These are the *historical*, the *philosophical*, and the *philological* sciences. All applied science is omitted. The current literature of applied science, engineering, etc., is fairly represented in the *Repertorium der technischen Journal Litteratur* issued by the Patent Office in Berlin, and by less comprehensive indexes in English. The current literature of history, of philosophy, and of philology is not, however, satisfactorily cared for by any existing comprehensive bibliographies. An index to the current literature of these sciences, if it could be undertaken by the Carnegie Institution, would be a most important and practical contribution to research. It would complement the International Catalogue. It might presumably be based upon the work of regional bureaus, precisely as is the International Catalogue, the Carnegie Institution assuming to it the

initiative and relation which the Royal Society has assumed to the latter enterprise.

(*c*) As to period: I have already indicated my opinion that for the Carnegie Institution, created to promote research, the most serviceable contribution in bibliography will be that which exhibits the recent and current literature rather than that which is retrospective. The investigator who is to advance the boundaries of knowledge will not, except as he is a bibliographer or historian of his subject, have much occasion for retrospect. In so far as he has occasion for such, he will require not a mere list of titles, but the actual books themselves. For these he must have recourse to a particular library or libraries. It is the duty of those libraries, through their catalogues, to furnish him with a statement of their contents. In the field covered by the International Catalogue, the Royal Society's "Catalogue of Scientific Papers," already covering the period 1800–1883, and proposing to cover also 1884–1900, is so nearly comprehensive as to render parallel attempts extravagant; just as in the field of medicine, for which the Index Medicus will cover the current literature, the Catalogue of the Surgeon General's Office Library forms for all practical purposes a comprehensive statement of the existing literature.

In considering undertakings more special, within a narrower field or a particular department of literature, the following considerations should apply:

I. In any subject in which there is active research, accompanied by a continuing literary record, a bibliography to be serviceable must also be continuing. A grant of a given amount will therefore, as a rule, be more effective if applied to a continuing bibliography within a narrow field than if exhausted upon a (periodically) limited bibliography within a larger field.

II. A bibliography differs from a selected list of titles, on the one hand, and from a catalogue of a particular collection, on the other. It attempts to be a complete exhibit of the literature of the subject. Such completeness exists in no single place or institution. A bibliography compiled at second hand can, however, be of but little authority. A bibliography which consists merely of brief titles, without explanation or analysis or an attempt to locate the material, can be of but meager utility. The preparation of a serviceable bibliography requires (1) direct use of the completest existing collections of material; (2) the most efficient bibliographic tools; (3) expert bibliographers, not merely specialists in the subject matter; (4) promptness and frequency of issue; (5) a form of publication which

will permit of distribution: (6) a form which will admit of the possibility of use by an individual investigator without great expense for accommodation and arrangement.

III. Duplication of bibliographic work is to be avoided. Coöperation is to be sought.

IV. The above considerations render inadvisable aid by the Institution to an undertaking which is isolated or fragmentary, which is not likely to be continuing nor practically exclusive within its field. It must, for instance, render inexpedient grants for the compilation or publication of a bibliography appended to a mere monograph on a particular subject, published as a commercial venture. The purpose of such an appendix can, as a rule, be better served by a selected list with discriminating notes than by a bibliography. Its circulation will be limited to that of the main work and controlled by commercial considerations, which are not controlling considerations with the Institution.

V. With the possibility of an undertaking which shall cover a large subject matter of concern to research, the Institution would, I think, be unwise to parcel its funds for bibliography by a number of small grants in aid of bibliographies of special subjects.

VI. *An aid which the Institution may render, of prime importance to all science, including the science of bibliography itself, would be to coördinate and correlate existing bibliographic sources, undertakings, and projects, to acquire and disseminate information which will exhibit the character of each, and the relations between them, and will prevent unnecessary duplication of effort and expenditure.*

I understand that the funds of the Institution available for bibliographic work during the coming year have been pledged in grants already made. I refrain therefore from expanding the above suggestions. I submit them now in explanation of the considerations which I should have in view in reporting upon any particular applications referred to me for recommendation.

NOTE. *Sources for Research in History and Sociology.*—These are scattered in institutions and archive offices here and abroad. To locate them with precision, to secure exact descriptions of them, and information as to the means and methods of access to them, and to publish these data for the information of investigators would be an obvious and important service to research. To secure transcripts of them and to concentrate these at some point most convenient to the

main body of investigators would advance the service into one of the highest utility.

Such an undertaking has been suggested as appropriate for the Carnegie Institution, and was mentioned in the deliberations of the Advisory Committee on Bibliography, but was deemed more appropriate for consideration and recommendation by the Advisory Committees on Historical and Economic Research. A proposal for an investigation into the sources for historical research at Washington has already been acted upon. Should similar investigations be undertaken of the sources in other places and abroad, accompanied by transcripts of important material, I should be glad, as Librarian of Congress, to submit some suggestions as to possible contributions to them by the Library of Congress which may result in a broader scope, a greater efficiency, and less expense to the funds of the Institution.

Very respectfully,

HERBERT PUTNAM.

REPORT OF ADVISORY COMMITTEE ON ENGINEERING

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: In response to the suggestion that the Advisory Committee submit such suggestions as are deemed helpful in determining the policy, methods, and acts of the Carnegie Institution, your Advisory Committee on Engineering herewith respectfully offers the following:

Preliminary.—The Founder has stated his intent, in the organization of this Institution, in simple and very definite terms, thus indicating clearly his primary purpose to be the promotion of the highest welfare of the people of the country, through scientific research, and incidentally to aid individuals who have been successful in that vocation, seeking mainly to help those who have most effectively helped themselves in the prosecution of that work.

The chief purpose of the Founder being, if possible, to secure to the United States of America leadership in the domain of discovery and "the utilization of new forces for the benefit of man," as stated in the deed of gift, it follows that such work in research as promotes the industrial arts and improves the system of production of the country is most important, in the views of the Founder. Scientific methods and scientific work in the field of engineering, that profession which devotes itself to the advancement of these arts, is directly in the line of the Founder's ideal.

The general scheme of the Carnegie Institution seems to contemplate something in the nature of a university, but devoted to research instead of education. A university as we commonly understand it is the head of an educational system combining the work of a museum with that of a school. The Founder of the Carnegie Institution seems to have recognized that there is another function not included in either of these. He has designed this Institution to be an investigator; in determining its functions and duties this should be remembered. The Institution proposes to supplement the work already done by the universities, but to supplement it on lines of its own. In any work undertaken by the Carnegie Institution its own identity should be maintained. Coöperation with others should only be directed to the prevention of duplication of work. The Institution should never subordinate itself to any other institution.

The aims of the Institution, as declared by its Founder in the deed creating the same, are:

1. To promote original research, paying great attention thereto as one of the most important of all departments.

2. To discover the exceptional man in every department of study whenever and wherever found, inside or outside of schools, and enable him to make the work for which he seems specially designed his life work

3. To increase facilities for higher education.

4. To increase the efficiency of the universities and other institutions of learning throughout the country, by utilizing and adding to their existing facilities and aiding teachers in the various institutions for experimental and other work, in these institutions as far as advisable.

5. To enable such students as may find Washington the best point for their special studies, to enjoy the advantages of the Museums, Libraries, Laboratories, Observatories, Meteorological, Piscicultural, and Forestry Schools, and kindred institutions of the several departments of the Government.

6. To insure the prompt publication and distribution of the results of scientific investigation, a field considered highly important.

These would seem to divide themselves into three groups. The first is the organization of the Institution itself. The second is analogous to the systems of fellowships established in a university. The third is publication.

The aims designated by the Founder as Nos. 1, 3, and 4 will come under the immediate direction of the Institution. The aims designated as Nos. 2 and 5 will come under a general supervision of the nature of fellowships. No. 6 will constitute the department of publication.

Institution.—The Institution should be conducted by a group of men, specialists in their own departments, capable of directing investigation and of recognizing the value of work done elsewhere. There should be among them that proper *esprit de corps* which belongs to any loyal faculty. Their primary duties are indicated by No. 1. The methods by which they will promote No. 3, "to increase facilities for higher education," must be developed as the Institution progresses. It would seem that at first little should be done in this way beyond putting the results of the researches which they encourage in such shape that they will be available for all who are seeking higher education. The trust deed apparently contemplates financial assist-

ance to other institutions, but this should be given only on the principle that the teachers and the faculties which are so helped should act with and be employed by the Institution. Even with this condition, the extension of financial aid to such institutions should be very carefully guarded.

Fellowships.— Two classes of fellowships appear to be contemplated. The first is for life, and its character is indicated in No. 2. The man who wins one of these awards would receive an income on which he could live comfortably, and this income would continue so long as he should devote himself to the work selected and should perform it for the Carnegie Institution. The second class, provided by No. 5, is of more limited period, and would consist of resident fellowships in Washington. To these it would probably be expedient to add a traveling class, which might enable students to pursue their specialties in other cities and countries.

Two conditions should be recognized in granting these fellowships: *First*, they should be given only to candidates of established ability; *second*, the holders must recognize that their work belongs to the Carnegie Institution, through which the results of it will be given to the world. The awards will be one of the most difficult tasks which the Institution has to perform.

Publication.—The duty of the department of publication will be to publish promptly the results of the work of the Institution, this including everything that may be done in other institutions which are temporarily acting in conjunction with it. Prompiness will be a prime necessity, as the value of the results of new investigations decreases rapidly with time. It will be expedient to issue two series of publications—the first in the form of advance sheets for immediate distribution among people known to be engaged in kindred studies, the second final memoirs in corrected shape.

As there is nothing in which time and money can be wasted more completely than in poorly conducted investigations, it will be important to classify different departments and subdivisions of departments which are recognized as proper fields of work for the Institution. Until such classification is made, it is very doubtful whether any specific grants should be given; furthermore, until the organization of the Institution is effected it is doubtful whether anything more should be done than to pass on the general expediency of propositions submitted, leaving final determinations for the future.

There is danger that, under the provision which permits subsidies, funds may be deflected from this Institution to purposes for

which they were not designed. All appropriations should be made for the purpose of promoting original research and not for education as it is commonly understood.

This committee is an Advisory Committee on Engineering. The greater part of an engineer's work is actual designing and execution. Neither of these is properly a subject for consideration or aid by the Carnegie Institution. Investigation and research which will aid engineers in the preparation and execution of their designs are proper functions of the Institution. Sanitary engineering must be based very largely on biological and other studies which properly come under other committees. The same may be said of mining engineering, the researches for which are largely geological and metallurgical. The specific lines of investigation and research which are left to this committee relate to the general subjects of energy and material.

The fields of scientific research in engineering are open to almost every department of science and include as their main divisions:

1. The physical characteristics of materials of construction, as cohesion, ductility, elastic limits, moduli of elasticity, their temperatures of fusion, volatilization, ignition, and decomposition.

2. Chemical composition, conditions of analysis and synthesis, methods of reduction of metals, of purification, of perfection in attainment of desired properties, etc.

3. Studies of methods and processes of production of the materials of engineering, and of manufacture, involving the employment of every art of the chemist and of the physicist.

4. Investigations of the work of the engineer in applied energetics and thermodynamics, including the character and value of fuel as a source of energy, its combustion, the transfer and storage of resultant energy in the working fluid employed in the engine, the nature and method of waste in the production and transfer of that energy, the process of energy—transformation in the heat engine—and similar studies in electric, pneumatic, and hydraulic energy utilization.

5. Research relating to the production of waste energy produced by friction.

6. Investigations of the class of those of Langley and Very on the relative efficiency of light producers, as of the fire fly and the candle or the electric light, determining the method of production and utilization of energy in the form of light.

7. Production of electricity from the potential energy of fuels without indirect transformation wastes. The process sought to be

revealed is that of a direct transformation of the potential energy of oxidizable substances into electric energy.

The field of research is too extensive to be described minutely, and the work in progress too varied and widely distributed to be indicated except in the most general way. For present purposes, however, it may be said that every important institution of higher learning, every professional school of engineering, a large and increasing number of business concerns, and several departments of government—general, state, and even sometimes municipal—are more or less well outfitted for such work and are engaged more or less extensively in its prosecution. The various technical associations and engineering societies are encouraging and stimulating research by affording opportunity to ascertain the immediately pressing questions to be thus solved, and by giving opportunity for the presentation and discussion with experts of the results obtained.

Even in the direction of finding men of genius, all these institutions and organizations are doing something and contributing, in some degree, toward providing time and opportunity for the display of scientific learning and of genius in research, discovery, and invention. But neither the universities nor the governments, even of the most advanced nations, are as yet finding ways to systematically identify the great investigators and men of genius in this work and provide them their opportunities.

Every school or college of engineering today includes in its curriculum far more extended courses of study and of laboratory work in chemistry, physics, and engineering science than is or ever was provided in any other department of instruction. Research in engineering is thus distinctly recognized as a fundamental element of progress. It is for these reasons that the work of the Carnegie Institution must probably find place very largely in connection with engineering and applied sciences which are its special basis. Both the attractions and the opportunities are large in this field, perhaps larger than in any other known department of human knowledge.

The fellowships to be instituted under the provisions of the deed of trust and all similar aids to individuals must evidently be carefully guarded against degeneration into the form of fellowships sustained by our institutions of learning, where the purpose is to aid education. Here the aim is research, and every appointee of the Institution must obviously be competent to make full return in fruits of scientific research for the assistance thus rendered. This neces-

sity will restrict these appointments to the few who have already exhibited genius and productive power and established habits of work in investigation. It will be particularly necessary to use caution in making appointments in aid of applicants without ample and well established records.

In pursuance of the policy of aiding those who help themselves and of making the income of the Institution go as far as is in any way practicable, as indicated in various ways by the Founder, it would evidently seem unwise to employ its income in the purchase of property, other than that contemplated in Washington, in the equipment of individual enterprises, or perhaps in any other way, generally, than the distribution of that income in small sums which may do most work by supplementing important efforts where a little additional aid may make complete and perfect a great work. Numerous opportunities will be found to secure comparatively important returns at little cost, and the innumerable applications for aid in all sorts of wise and foolish ways which come before the committees give evidence that only by carefully and safely providing for the most important objects in view can serious waste of funds be avoided.

The relative importance of a department of research which directly promotes the progress of the race industrially is so obvious and so enormous that your committee feel justified in submitting the proposition that the largest practicable support should be extended to all work bearing upon its extension and maintenance.

The first step to be taken in systematically preparing the way for the great work to be carried on by the Institution and insuring the highest possible efficiency of the operation of the latter would seem to be the systematic gathering of information relating to men, equipment, and facilities already existing, ascertaining where a deficiency exists, where ample provision is already made, what are the deficiencies and how they may be best remedied, what is the best way to make immediate and productive use of those which are in one or more lines substantially complete and satisfactory.

In this work it is to be presumed that the advisory committees may effectively aid. Each in its special department may find the men, discover the evidence of genius. By the issue of circulars of inquiry it will be easily possible to ascertain substantially what apparatus and facilities every institution of learning, every industrial enterprise, and every contemporary investigator has under control in the field assigned; thus, as a supplementary result, learning just what

in each case may or should be done by the Institution or others to make the inventory complete for purposes of important research.

These methods have all along been obviously proper introductories to the work, but this committee has not felt authorized to enter upon their promotion until specifically authorized so to do by the Trustees of the Institution as part of a general and well considered scheme approved by them. The committee has confined itself thus far to the consideration of matters referred to them by the Executive Committee.

Recommendations.—The specific *recommendations of the committee* are, following the scheme of the Founder:

1. That promotion of original research, rather than education as commonly understood, be made the primary object of the Institution.

2. That this object be promoted by—

(*a*) The discovery of the exceptional man and assistance to him to enable him to make the work for which he seems specially designed his life work.

(*b*) By increasing the facilities for higher education by ascertaining just where those facilities are most likely to prove fruitful of good to the country and to the world, by discovering what facilities already exist, and, finally, the best way of supplementing well established and safely organized equipments for advanced and professional education and scientific work.

(*c*) By utilizing and adding to existing facilities of the universities and other institutions of learning throughout the country and aiding their teachers in research.

3. That special aid be extended to fellows and other individuals for stated work; these fellows to be temporary, permanent, traveling, etc., in order that all existing sources of knowledge may be utilized to promote research.

4. That a permanent Carnegie Institution organization be established, consisting of—

(*a*) Committees, general and advisory.

(*b*) A faculty of a rather limited number of individuals, who are specialists in the various departments, with a staff capable of conducting the routine business of the Institution.

5. That few, if any, Carnegie laboratories, workshops, or schools be established as parts of same; nor should those in existence be taken over, thus saddling the Institution with management duties in widely separated places and placing it in competition with existing institutions.

6 That special Carnegie Institution publications shall promptly give to the world the results of the Institution's work—that is, the work of its faculty, fellows, and aided individuals and institutions. The circulation of these publications should be wide, to reach all deserving persons interested, and at a nominal price. It is suggested that later a plan can be devised to supply promptly and thoroughly edited science abstracts, as filling a most important need.

Respectfully submitted,

R. H. THURSTON, *Chairman*,
W. H BURR,
GEORGE GIBBS,
GEO. S. MORISON,
CHAS. P. STEINMETZ,
Committee.

NEW YORK CITY, *October 24, 1902.*

REPORT OF ADVISORY COMMITTEE ON PSYCHOLOGY

CONTENTS.

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: In preparing this report certain general considerations occurred to the mind of your committee:

I. GENERAL CONSIDERATIONS.

Psychology stands over against the natural and physical sciences, as underlying what may be grouped together as the sciences of man. This latter group of sciences includes anthropology broadly defined, history, and the mental sciences proper in their association with the moral and social sciences. It has become more and more the recognized view that exact work in psychology must precede and furnish the foundations of the scientific structure in each of these branches of knowledge. Accordingly, if we should say that physics and chemistry were the fundamental physical sciences, and that biology, used to include both botany and zoölogy, was the fundamental science of life, psychology would hold a corresponding place in relation to the entire group of what we have called above the sciences of man. Too great importance, accordingly, cannot be attached to the treatment of this subject in any scheme of the sciences adopted by the Carnegie Institution, which proposes to encourage and provide for research in science generally. The fact that the physical and natural sciences have already had a richer development should not prejudice the claim of psychology to the fullest recognition; on the contrary, such a fact only gives increased emphasis to the needs of this department, provided the provision made for it can be both definite and fruitful.

The history of this science in the last twenty years has been one of remarkable progress. Psychology has become a positive science,

and the actual results of the application of the experimental method are, we venture to think, greater, relatively speaking, than those of any other science in the corresponding period. Sufficient advance has been made to justify the establishment, in the great universities, of laboratories and other material aids to psychological research. The literature of the subject, as listed in *The Psychological Index*, reaches for 1901 an astonishingly large number of titles, and two great reviews are regularly published in this country alone, one of them also finding it necessary to issue large supplements for the printing of extensive pieces of research.

Certain more general features of contemporary psychological work, when viewed with reference to such a grouping of the sciences as that suggested above, may be pointed out In the first place, the development of psychology is necessary for the sound solution of most important problems in the physical and natural sciences. In physics, for example, matters of direct psychological import come up for treatment, such, for example, as the question of the personal equation in astronomy, the question of the normal function of the senses in physical observation, the question of the extent and psychological justification of the various methods of research—all problems in which, as history shows, final solutions have waited upon the results of psychological criticism and research. In the biological sciences the same is true, but to a greater extent. The development of genetic psychology in recent years has been perhaps the most important modifying influence in general biological theories. The problem of evolution is now as much psychological as biological, and the biologists themselves are prepared to ask the coöperation of expert psychologists in their most difficult undertakings. Apart from this relation of psychological investigation to that in the physical and natural sciences, the direct value of psychology in connection with all the sciences of man remains over—a fact which in itself is sufficient justification for the most liberal endowment of psychological research.

The principal function, as we conceive it, of such a new establishment as the Carnegie Institution in relation to psychology should be that of *unification*—the function which is of the first importance in reference to the relationships of all the sciences. The development of psychology, rapid as it has been, has been along distinct lines. We have today no less than five general undertakings, all yielding fruitful results and each pursued by more or less independent methods, namely: 1st, Laboratory Psychology (including

both Experimental and Physiological Psychology); 2d, Genetic or Comparative Psychology (including Zoölogical and Anthropological Psychology); 3d, Social, with its important branch, Educational Psychology; 4th, Statistical Psychology (including questions of mental variation, heredity, types of mind, etc.); and, 5th, Pathological and Abnormal Psychology—the investigation of diseased and abnormal minds. The course of development in these somewhat distinct and separate lines has been so self controlled that proper relationships do not exist among these departments. It is now of the most extreme importance—and it is indicated above as one of the most evident functions of the Carnegie Institution—that there should be an agency for the better unification of researches in these different fields. This, and with it the direct encouragement of research in each of the great subjects mentioned, with their subordinate subdivisions, constitute, in the opinion of your Committee, the main topics for recommendation in this report.

The presentation now made of the present condition of psychological inquiry may be supported by reference to a recent undertaking in Paris. I refer to the foundation in 1900, in connection with the International Congress of Psychology, of what has been named the General Psychological Institute. This association, formed under the patronage of an international committee, upon which your present reporter was asked to serve, has for its explicit objects: *First, the advancement of psychological science*; and, *second, the unification of the branches of psychological work*—the two objects which the present report is also emphasizing. The importance of the function of such a general institution in unifying the results of science is seen, or may be seen, in the following quotation from a recent bulletin issued by this French Psychological Institute: "The branches of psychological science [says this report] appear to be pursued in too great independence of one another. Properly speaking, they should be so closely connected that it would be impossible to make a profound study of the facts of any one of them without thorough knowledge of the body of results from the others. How, for example, can we study the psychology of children without knowing the work that has been done on such questions as that of the general psychology of suggestion? Or how study the mental condition of the alcoholic inebriate without knowing all the results of research which bear upon the psychic effects of drugs? In short, *solidarity, in this science, has become a great need.* The tendency to form international congresses, and, in particular, the work of the

psychological congress, clearly shows the need and the utility of this solidarity. Partial groups of workers can be but temporary. There is need, therefore, for a permanent center, where the different branches of psychological science can be brought into continual and helpful contact. Then only will all the scattered efforts made in different places be made to converge upon special, broader points of study, with the result that light will be thrown upon them all by this effort at synthesis. This is precisely the controlling idea in the foundation of the General Psychological Institute."

This report is issued by a board of expert psychologists. It may be added, however, that this French Institute depends upon public subscriptions and dues of membership, and has so far been able to do little beyond the issue of programs and suggestions to single workers.

In the same connection the present reporter may also cite the policy of the recently issued *Dictionary of Philosophy and Psychology* of the Macmillan Company—an international coöperative undertaking of which he has acted as editor. The policy of this work, devoted to the mental and moral sciences and to the criticism and theory of science in general, is shown in the fact that psychology and psychological research have been made the center of the entire undertaking; and, furthermore, it appears that in this work it has been thought necessary to include the main conceptions of both physical and natural science—so intimate has become the relation of psychology to these other disciplines. The following quotation from the general preface to that work may serve to show its point of view. It reads: "Psychology is the half way house between biology, with the whole range of the objective sciences, on the one hand, and the moral sciences, with philosophy, on the other hand. The claim to this place made by psychology today is no more plain than is the proof of it, which the results in this department of research make good. The rise of experimental and physiological psychology has caused the science to bulk large towards the empirical disciplines as it always has towards the speculative; and the inroads made by psychological analysis and investigation into the domains where the speculative methods of inquiry were once exclusively in vogue, render permanent and definite the relation of that side as well. In biology, in sociology, in anthropology, in ethics, in economics, in law, even in physics, the demand is for sound psychology; and the criticism that is making itself felt is psychological criticism. * * * It will be found, therefore, that

it is upon the psychology of this work that most of its lines converge."

Again, in the following quotation the emphasis laid upon science by the editors of that work is brought out: "It is one of the safest sayings of philosophy at the close of the outgoing century that whatever we may become to end with, we must be naturalists to begin with—men furnished with the breastplate of natural knowledge. We must know the methods as well as the results of science. We must know the limitations of experiment, the theory of probability, the scientific modes of weighing evidence and of treating cases. Lack of these things is the weakness of many a contemporary writer on philosophy. Such a one criticises a science which he does not understand, and fails to see the significance of the inroads science is making into the territory which has so long seemed to be exempt. Note the application of biological principles, in however modified form, to psychological facts, the treatment of moral phenomena by statistical methods; and these things are but examples. These topics are becoming of special importance to the psychologist, the moralist, and the student of life."

Two general utilities are therefore pointed out by your committee as most important, namely: *First, the encouragement of definite scientific research* in psychology; and, *second, the unification, in what the French call solidarity, of the different departments of psychology.*

In the opinion of your Committee, both of these objects can be best subserved *by the establishment at Washington of a department of psychology in immediate connection with any other scientific establishments which the Carnegie Institution may found.* The provision for this science will then rank with that of certain other fundamental sciences in the appropriations of the Institution.

In such an establishment psychology should properly have two units from the entire number of units of appropriation in the scheme of the sciences: that is, if ten divisions of science be recognized as having claim upon the income of the Institution, it is the deliberately formed judgment of your Committee that psychology should be assigned two-tenths of the entire income. This, however, does not mean that researches which are only of psychological value would be so largely entered upon, for one-third or one-half of the work undertaken would be in joint control of psychology with some other science or sciences. This will appear from a concise statement of the principal problems of the several departments already mentioned.

II. Most Important Practical Undertakings.

Making the foregoing the basis of our more detailed suggestions, the two great divisions of appropriation may be these: (1) The support of a central institution with the several departments enumerated below; and (2) the establishment of a fund for grants and subsidies of various sorts for the direct encouragement of psychology throughout the country.

These two objects can, in the opinion of your Committee, be well combined in a way which will at once stimulate the psychological work of all the universities and, at the same time, supplement and further them.

It is assumed without discussion that the Institution is prepared to undertake grants for special researches. Your Committee is of opinion that a sum of from $5,000 to $10,000 can be profitably employed at present for such grants in psychology This sum, however, it is evident, should be extremely flexible.

To enter into some details of recommendation as to the constitution of the department of psychology, your committee suggests in outline the following scheme:

First. A department for *Genetic*, including *Zoölogical* and *Anthropological Psychology*. In the classification which we are now presenting, the principle of which is no less economy than utility, two bureaus are here included—that for what is generally called *Comparative* or *Zoölogical Psychology*, and that for *Anthropological Psychology*.

The former of these aims to carry the investigation of the mind or consciousness into all of its manifestations in the animal world. It recognizes the great doctrine of evolution and its debt to the interpretation of the series of animal minds in terms of their genetic descent. We thus have the problem of *mental morphology* as it has been called by a prominent biologist—a problem which is as wide in its reach and as important in its solution as the great problem of morphology is to the biologist, since the rise of the modern Darwinian theory. Important beginnings have been made in the investigation of the animal mind; but the hindrances which have presented themselves to individuals and institutions in this research have been almost insurmountable, seeing that such investigation requires the keeping of typical animals of various habit, size, food, and care, and the breeding of these animals for considerable periods, in order that observations may be systematically carried out. Furthermore,

the observations themselves require the constant presence of trained observers, together with the carrying out of systematic and exact measurements. All this is an undertaking of such complexity and magnitude that nothing but a central establishment, where the conditions may be made constant and the observers free from other undertakings, would be adequate. This should be undertaken as part of the project, if such be carried out, of an experimental farm established for the investigation of biological and genetic problems. Failing that, however, a psychological station of the sort mentioned might well be established, at very moderate cost, in connection with one of the larger zoölogical parks of the country, to begin certain well planned researches. Washington presents facilities of this sort, and your Committee urgently recommends that this project be at once entered upon. By doing this the Carnegie Institution would be absolutely the first agency in the field for accomplishing work for which the time is ripe, and for which other agencies are strenuously exerting themselves in a partial way under their peculiar limitations. The coöperation of Psychology and Zoölogy in this matter is most desirable.

The second department of research under this general head of Genetic Psychology is that known as *Anthropological Psychology*. As the term indicates, this department investigates the psychological processes—in short, the minds—of races and peoples at every stage of culture. The urgency of the undertaking from the anthropological side has been recognized and, in many instances, met by private provision in recent years. It only needs stating that many savage peoples are rapidly disappearing from the earth, carrying with them all that is living of their customs, institutions, superstitions, religions, and mental parts in general. This only needs to be stated to convince the intelligent sympathizer with science that it is a principal duty of the modern world to collect facts in chosen regions while these facts still remain to be collected. Hitherto, however, the anthropologists have worked largely without the coöperation of psychologists. Despite the best efforts of anthropologists and their full sympathy with the psychological problems, their expeditions have not been properly manned for the carrying out of mental researches. Such work may be illustrated by the English expedition sent out by Cambridge University to the Torres Straits, with which a psychologist was sent having a fairly good equipment. The published results fully justified expectations. Psychological work was also attempted in connection with the recent

Jesup expedition sent out from New York. It is an undertaking of first rate importance which falls as an opportunity to the Carnegie Institution—a task which, like those already mentioned, will remain largely undone unless such a central institution undertakes to do it. The principal appropriation in this field would be to anthropology, but a trained expert and his adequate equipment would belong to psychology.

Second. A second bureau is recommended, under the terms *Educational and Social Psychology.* Educational psychology has both its theoretical and practical side. Theoretically it aims to formulate the laws of operation and function of the mind at different periods of growth, and with the recognition of different types of temperament and individuality. It thus aims to supply that knowledge of the growing individual which is requisite for his proper education and training from earliest childhood to maturity. The lack of such a body of knowledge based upon actual tests, measurements, and statistical observations, has been the great obstacle to the progress of educational science. On the practical side, such a department addresses itself to the use of such knowledge of the mind in arranging and controlling the actual work of the schools. The classification of school children, the differences of the sexes, their relative maturity at different periods, the proper distribution of time to different subjects, the adjustment of the body in periods of mental application, the laws of fatigue and recovery, the testing of the senses, the hygiene of the mind in the close social relationships in which children and youth are thrown, the systematic carrying out of tests and measurements upon college students—all these are of the important practical matters which affect the education of our youth. These matters can not, in the opinion of your Committee, be undertaken in a single school or university, nor upon a single type of individuals. They require large numbers of observations, repeated from year to year, the most careful devising of experiments to reach the mind without interfering with it, and other equally broad arrangements which can only be carried out by a central institution provided with adequate facilities. Washington suggests itself as possibly unsurpassed in the material and opportunities which it would present for the prosecution of educational psychology. We suggest that this work should have the coöperation of the Government Bureau of Education, and it should be expected to be of material aid in the work of that Bureau.

The Social Bureau in this department, it is easy to see, should be

closely affiliated with the Educational. It should deal with a large range of subjects and materials, having broad social questions before it. A new department of inquiry, called *Criminology*, has recently been developed, principally in Italy and France, which aims to determine both the social and the individual causes and conditions of crime. The value of such a science must depend essentially upon the soundness of the psychology which it adopts. Hitherto the psychology has been the debatable ground in the entire movement, and criminology has remained the work of individual theorists and observers, while waiting for authoritative collective work upon a sufficiently large number of data. Besides the criminal, other social classes should also be investigated, and the psychologist has an unlimited field for work of great importance in determining the conditions of what has been called "collective" thought and action. The psychology of men in groups, such as the lynching party and the street riot, the reasons for the differences between individual actions of individuals and the actions of masses, the analogies between the performances of what is called the human *mob* and those of minds of lower grades—the animals, human defectives, etc.—these are all important problems. Such a bureau should have at its disposal the resources of a statistical establishment, and command well trained computers to treat the data which are secured. Taken together with Educational Psychology, this department of work makes, in the mind of your Committee, an urgent claim upon the Carnegie Institution next to that of Zoölogical and Anthropological Psychology.

Third. There should be established a well equipped laboratory providing for research in both experimental and physiological psychology—what may be briefly called *Laboratory Psychology*. This laboratory should be so equipped as to provide much of the apparatus required in each of the separate bureaus mentioned. It should be located in a building with the bureaus already mentioned. In this way the expense of equipment would be considerably diminished. In order not to duplicate existing laboratories too much, the equipment should grow gradually as special researches in all the psychological departments may require. For this reason we put this *third* in order.

This department, called above that of Laboratory Psychology, includes two distinct branches of research, both employing exact methods and requiring an equipment of apparatus—Experimental Psychology and Physiological Psychology. By experimental psy-

chology proper is meant the investigation of the mind by experimenting upon normal individuals through the ordinary avenues of sense, or, in other words, through stimulations to the nervous system. It includes the experimental investigation of sensation in all its kinds, namely, vision, hearing, touch, muscular sensation, etc. In this department considerable advance has been made in recent years. Delicate problems, for example, of the theory of vision, including questions of space perception, of color vision, of visual association with other sensations, are now profitably taken up, and standard apparatus for the investigation and demonstration of such phenomena has in some cases been devised. The practical utility of an equipment for experimental psychology, in connection with the Carnegie Institution, would appear in the prosecution of such subjects as that of the investigation of color perception and its defects in connection with colored lights, railway signals, etc., the training of the senses, etc., where at present the greatest confusion and disorder prevails. Citing these as instances, merely, in but one of the departments of sensation, where many of equal importance might be cited, one is able to see the utility of a well equipped laboratory for such research, where, if necessary, the work on any single problem in any department may be pursued.

In physiological psychology, on the other hand, the main problem is to investigate mental conditions in connection with the physiological processes which accompany them, and more particularly in the variations which are presented by accident, disease, etc., or which are directly arranged by the experimenter. Here the science of what may be called neuro-psychology or psycho-physiology has been developed. To cite in this connection also a single line of research, we may refer to the localization of functions in the brain and the remarkable advances in the physiology and pathology of speech. This work has constituted one of the most interesting pages in the history of modern physiology and medicine, no less than a contribution of extreme importance to our understanding of the relations of mind and body. Areas have been located in the brain, as, for, example, that of the function of speech, so definitely and presenting such marked mental symptoms that the surgeon is able by a direct operation to reach the spot, and cure the patient. Our point of emphasis here is that the symptoms are mental, that there are variations or derangements of sensation or movement, as in the case of speech, and the experimental knowledge acquired and the mode of treatment really belong to psychology. The equip-

ment of a laboratory, where direct experiments may be planned to clear up the many problems which are still obscure, would be of untold utility, not only to the sciences of psychology and brain physiology, but also to medical practice.

Fourth. A fourth department of research, that of *Mental Pathology*, is devoted to the investigation of abnormal conditions of consciousness. Its establishment would involve for its adequate equipment so large an appropriation and such extensive resources that it is probably not within our reach at the present time. Your Committee recommends, however, that the bureaus described above for social and physiological psychology should take upon themselves certain more restricted researches in this field. The investigation of the abnormal and defective of certain sorts may very profitably be associated with the gathering of general social statistics. The observation of defective children, for example, in asylums for the deaf, dumb, and blind, and the carrying out of certain tests and measurements upon well determined types of mental diseases in hospitals and asylums would be a work which might well be undertaken, and with profit, by that bureau. Material for such study exists in institutions at Washington, and there can be no doubt that the coöperation of these institutions could be counted upon, as can also that of the schools and, as is said above, of the Government Bureau of Education, for the furtherance of these researches.

These intimations may suffice in the present report as a description of the sort of work which should be undertaken in these several fields. The inspection of this brief list, however, cannot fail to impress the reader with the *need of coördination and of coöperative work by all of these departments under some general direction.* The object of it all is to advance and apply the science of the mind, and that science is most adequately advanced when the results from these different departments are assimilated, digested, and applied in practical life. There should result, therefore, as the work progresses, if it be well done, an appreciable advance of what we call general or systematic psychology. The work of the Institution should show itself in future generations in the theories of the mental life as a whole, in the teachings of instructors, and in the text books used in universities and elsewhere. The important function of the central establishment is, therefore, to be kept in mind in all the work of the departments, and these latter should not be so distinct or locally separate from one another that workers in one of them may not, under suitable conditions, call upon another or be

called upon by the other for aid. In the way of indicating the best results to be reached by the work of all the bureaus in connection with one another, the considerations already presented in favor of a central unifying establishment, with functions such as providing lectures and issuing reports, etc., may be again emphasized.

With so much description of the more general fields of work in which the Carnegie Institution may do most for psychology, we may point out certain subordinate urgent ways in which particular lines of work may be undertaken in connection with the central establishment:

(*a*) A bureau for the *manufacture and sale*, at greatly reduced prices, of all sorts of *apparatus* required by scientific men.

(*b*) A bureau in which there should be provision for the training and support of *expert computers*, who would be at the service of the educational institutions for computing and statistical work. Such a bureau is a necessity in many departments, and a single establishment would suffice for all the sciences.

(*c*) A bureau for *anthropometric and psychological tests and measurements*. The object of this bureau will be the establishing of certain standard tests and measurements, both of psychological and physiological character, to be carried out upon individual classes, such as college students, school children, and primitive peoples, savages, and others in connection with the departments already described. A good beginning has been made by a committee of the American Psychological Association in devising and arranging a set of standard tests.

(*d*) This department should join in the maintenance of a *printing establishment* for the manufacture of scientific works of all sorts at liberal and reduced rates for scientific institutions, societies, and agencies generally, and for its own printing in all the departments.

(*e*) There should be in connection with the central establishment a *literary bureau* for the conduct of such publications as may be necessary. This would include the publication of reports, bulletins and literary aids of all sorts, such as bibliographies, translations, etc. We especially point out the need of an agency for securing the *translation and publication of important foreign books* and memoirs which private publishers do not find it profitable to bring out.

III. Specific Recommendations.

Advisory Committee.—In addition to the officials indicated below for specific duties in connection with the central department of psychol-

ogy, when that is established, your Committee deems it very important that there should be a permanent Advisory Committee associated with the Carnegie Institution for Psychological science in its broadest definition. Such a committee would be a medium of communication between the Board of Trustees and the officers of the Psychological Department. The principal function of this Committee would be to give, in all matters concerning psychology, expert judgment and advice. It would pass upon all proposals of a specific kind which come before the Trustees. It would be competent to take the initiative in making recommendations as to new undertakings which the condition of the science from time to time seemed to render advisable. It would judge as to the qualifications of particular beneficiaries of the funds of the Institution, make recommendations of grants, and suggest appointments in connection with the staff.

It should be charged to find the *exceptional man* and suggest ways in which he may be profitably assisted. In view of these functions, exercised with reference to the entire body of researches in psychology, the chairman of this Committee should be as *broad and tolerant* an expert psychologist as can be secured, rather than one versed only in some one branch of the subject. At the same time the Committee should be numerically small. The chairman may also be responsible Director of the Department.

The chairman should also represent the Committee in conference with representatives of the Board, and also of the advisory committees in the other sciences, so far as such may be constituted. This latter form of conference, namely, among the chairmen of the different committees, seems to us to be of great importance. By such conference the different departments of science would be kept in coöperation, and the benefits of this coöperation would accrue to science generally.

IV. Equipment and Maintenance of Psychological Department, with Estimates of Cost.

In order to be definite in our suggestions as to the most urgent lines of procedure, your Committee recommends the following scheme:

(1) *Building.*—In case there is established in Washington a central institution affording complete accommodations for such of the sciences as find central bureaus necessary, psychology should be

given a special structure, the construction, arrangement, division of rooms, etc., being in charge of one or more expert psychologists.

(2) *Equipment.*—

	First Year.	Second Year.	Annual appropriation.
(*a*) Bureau of Genetic (Zoölogical and Anthropological) Psychology	$10,000*	$5,000	$5,000
(*b*) Bureau of Educational and Social Psychology, to include Bureau of Tests and Measurements	5,000	5,000	5,000
(*c*) Laboratory for Experimental and Physiological Psychology Over and above provision for building, we recommend an appropriation of.......	10,000	10,000	5,000
Totals...	$25,000	$20,000	$15,000

(3) *Salaries and Administration.*—We recommend that certain salaried positions be created for the maintenance of the Psychological Department, distributed with a view to the work of the branches or bureaus mentioned above. The *Chairman* of the Advisory Committee should have a salary, understanding that he is to give attention to all details of grants, projects, etc., which come to the attention of the Institution in psychology. There should be a *Director* at the head of the department as soon as the scheme of bureaus is realized, who should be responsible for its general conduct and administration, who should preferably live in Washington, or, if connected with another institution, should give at least half his time to the affairs of the Carnegie Institution. With him should be associated officers known as *Professors* or *Heads of Departments* or the equivalent, each charged with the control of one of the great divisions of the work as mentioned above, namely, one for *Genetic Psychology*, one for *Educational and Social Psychology*, and one for *Laboratory Psychology*. No recommendation is made as to the institution of a department of Pathology at present. These Carnegie professors, if we may call them so, will be three in number, one of them, however, being already provided for in the person of the Director. In other words, the Director's duties and qualifications should comprise the conducting of one of these three departments.

*This figure assumes the need of constructing some sort of houses, etc., for animals. It may be provided for, in whole or in part, in the Zoölogist's recommendation.

The scheme of expenditure which this arrangement will involve may be indicated as follows:

(*a*) Salary of Chairman of Advisory Committee, $3,000 (besides office and other necessary expenses of the committee, and to be paid only in case he holds no other position in the Institution).

(*b*) Salary of Director, $6,000 or $10,000 (according as he gives a part or the whole of his time). Great flexibility should characterize the arrangements in order to secure the right man.

(*c*) Salaries of two heads of departments or professors at $3,000 or $5,000 (according as they give part or the whole of their time)—$6,000 or $10,000. It would seem quite feasible for a university professor to conduct one of these departments while still serving his university, provided competent assistants or associates be given him.

(*d*) Salaries of staff officers or associates—one or two for each of the three departments, at $2,000 to $3,000 each: First year, $6.000; second year, $7,500; third year, $10,000.

	First year.	Second year	Third year.
Totals....	$20,000 (about)	$25,000 (about)	$30,000

Uniting these two estimates for *equipment and maintenance* and for *salaries*, we have as follows:

	First year	Second year.	Annual appropriation.
Equipment and maintenance..................	$25,000	$20,000	$15,000
Salaries	20,000	25,000	30,000
Totals..........................	$45,000	$45,000	$45,000

That is, an expenditure of $45,000 per year, divided as indicated above, will give adequate equipment and permanent maintenance to such a department as we have sketched.

(4) *Special Objects.*—In addition to the foregoing, we recommend the establishment of a fund for certain special objects, the total to be very flexible. The objects to be covered by this fund are, in our opinion, these:

Grants and subsidies......................................	$10,000
Research fellowships (by whatever name they may be known)........	6,000
Lectures, publications, etc..........................	4,000
Total	$20,000

In explanation of this class of special objects, we may say that by Grants and Subsidies we understand particular cases of research

which the Advisory Committee may find it wise to recommend. This would include all applications which come to the Institution and are favorably considered under the terms of the deed of trust.

By *Research Fellowships* your Committee intends the establishment of certain bursaries, to be awarded to mature and most capable individuals, to enable them to carry out important researches at the Institution or elsewhere, as may be determined, for one year or a term of years. These should be awarded on recommendation of the Advisory Committee. It is thought that six such bursaries, yielding $1,000 each, will, at any rate for a certain number of years, be sufficient.

The third item, *Lectures, publications, etc.*, is one which your Committee recommends with a view to adopting means of furthering in a public manner the interests of the science. We think it would be extremely helpful if experts should be secured to give at intervals, in Washington, lectures devoted especially to topics of the interconnection of the sciences and of different branches of science, with *résumés* of the progress of science, and to topics which would, in general, serve to unify the work of the Carnegie Institution. This should be undertaken in coöperation with other departments.

Summing up the recommendations made in this section of the report, we reach the following condensed statement:

Recommendation for equipment, maintenance, and salaries, as given above, annually	$45,000
Recommendations for special objects, as given above	20,000
Total annual appropriation	$65,000

The total recommendations thus involved amount to from $60,000 to $65,000. The maximum sum recommended, namely, $65,000, is, in the mind of your Committee, by no means excessive.

(5) *Partial Schemes.*—Partial, or lesser provisions, in case reduction is imperative, could be carried out by omitting one of the principal bureaus or departments. In this case the *Equipment of the Laboratory* may be reduced to the essentials of the special bureaus and many researches undertaken without such full equipment. This gives a reduction in salaries and equipment to the amount of $10,000 a year. This reduction would make the entire requisition for psychology $50,000 per year (when the whole scheme has been put in operation).

V. Immediate Procedure.

With a view to proceeding as financial and other conditions may allow, on the lines now suggested, your Committee makes the following recommendations in regard to immediate procedure:

(1) It is urgent that a permanent Advisory Committee be constituted, as already indicated.

(2) An appropriation of $5,000 is recommended for the preliminary work of a bureau of *Psychophysical Tests and Measurements*; to extend and apply the work already done (by a committee of the *American Psychological Association* and by private individuals) principally upon university students. The procedure should be left to the Advisory Committee on Psychology.

(3) An appropriation—jointly with Zoölogy—for the study of animals. Immediate procedure is strongly recommended, a joint recommendation being secured from the Committees on Zoölogy and Psychology.

(4) Special grants as per report on specific applications.

We present herewith, in support of the general lines of our recommendation, copies of letters giving the views of certain psychologists—themselves representative men, who also represent institutions having important psychological departments.

These letters were sent to your Committee in response to the chairman's request, with which in each case a list of suggested undertakings was inclosed. A copy of this list is also appended. These letters give the judgment of leading authorities as to the relative availability of the undertakings upon which the Carnegie Institution might enter in this department. It will be seen that the opinions expressed are in the main coincident with the recommendations made in this report, and also that such a scheme as that now suggested is, in their opinion, not at all in conflict with the interests of their university departments. More than that, in their opinion, the institution of a central establishment for psychological work in Washington will help and stimulate, rather than compete with and discourage, the psychological work now being done in the universities.

Respectfully submitted,

J. Mark Baldwin, *Chairman.*

October 31, 1902.

APPENDIX TO REPORT OF COMMITTEE ON PSYCHOLOGY

[*Projects in Psychology on which certain expert opinions were asked.*]

1. Unification of departments of psychology; bureau for lectures, conferences, etc., in Washington.
2. Laboratory for experimental and physiological psychology in Washington.
3. Bureau for educational psychology, adjunct to above.
4. Bureau for tests and measurements, adjunct to above.
5. Station for the study of comparative psychological undertakings in connection with biological experimental farm.
6. Bureau for statistics and compilation to train and supply expert computers to all the institutions.
7. Bureau for the study of mental pathology, adjunct to an establishment in general pathology.
8. Establishment of fellowships for research, such as fellows to work in the several bureaus in Washington.
9. Grants and subsidies to particular individuals for research.
10. Preparation of a bibliography for psychology.
11. Establishment jointly, for all the sciences, of a bureau for cheap printing, lithography, etc.
12. Possible provisions for philosophy and logic (no definite proposals have as yet been made).

From the replies to requests for opinions on the above projects the following have been selected to form a part of the report of the Committee on Psychology.

[*Dr. George Trumbull Ladd, Professor of Philosophy, Yale University, to Mr. Baldwin.*]

The purpose and opportunities of the Carnegie Institution are so unique that there is little in the previous history of education and scientific research which can serve as a very exact model or altogether safe guide. I think. therefore, that its first years should be very largely experimental; that the Institution should, so to say. feel its way cautiously to its best and most effective service for the advancement of knowledge and the practical benefits of increased knowledge, in this country and throughout the world. Its efforts, so far as concerns the aid it can render to psychology and to the psychological sciences should, in my judgment, be arranged in the following order of precedence:

1. The unification and supplementing of the work of research done by the universities and individuals best equipped for such work. This branch, however, can not be defined in particular at the beginning. But as instances of definite assistance in the direction of unification I will mention the following:

(*a*) The creation of some sort of a bureau of information, so that the different persons engaged in psychological researches can know what is being done, and what has been done, by others toward the solution of the same problems. It has been my experience that much time and energy are often wasted through failure to obtain such information.

(*b*) Some arrangement by which friendly criticism and suggestion can be had with a *minimum* risk from those jealousies, misunderstandings, and even misrepresentations from which it is so difficult to keep free even our best scientific work.

(*c*) Perhaps, still further, more adequate provision for the efficient distribution of apparatus, books, pamphlets, etc., that are either necessary or helpful to the investigator, but which he could not obtain without the assistance of the Institution.

Such ways of unifying as these are, of course, at the same time ways of supplementing the work of particular universities and of individuals.

2. Some sort of a central plant seems to me to be the next most pressing need. I should not, however, think it wise to spend immediately a large sum in buildings or in the purchase of expensive apparatus for experimental purposes. Indeed, a certain rather limited amount of space allotted to the uses of a psychological labo-

ratory might be, and I think would be, all that is desirable for some years to come. The most important equipment for such a laboratory is—

(*a*) A workshop in which apparatus of the most useful sort, and perhaps also some of the highest quality of finish and exactitude, can be manufactured.

(*b*) This workshop will be comparatively useless unless there are two or three mechanics connected with it who can have the very special training which such manufacture involves.

(*c*) Some oversight from a trained expert in this branch of psychological research.

Besides the workshop, the laboratory should have a few rooms which might be from time to time, at no great expense, adapted to the uses of those conducting researches at the central building of the Institution.

The uses to which the laboratory should be put are:

(*a*) The providing of those who are conducting researches of a large general sort—*e. g.*, in anthropological or pedagogical lines—with the necessary equipment of apparatus for their experiments. Such experiments imply that the traveling expert takes his mechanism along with him.

(*b*) The conductors of pieces of research in the rooms of the laboratory, or in the city of Washington, can then have their apparatus constructed, tested, and readjusted under their direction.

(*c*) By and by, if not soon, the laboratory of the Carnegie Institution might be able to supply experimenters in this country with as good mechanism as can now be obtained in France or Germany, of the special sort adapted to experimental psychology, at cheaper rates, or even by way of a loan.

Beyond these uses I do not deem it wise for the laboratory of the Carnegie Institution to aim to go until at least the rather delicate problem is practically solved of its really supplementing rather than harming the work of the laboratories of the universities.

3. Next in importance I should place the commissioning and equipping of experts in the matter of making psychological observations, tests, and measurements, computing results, etc. No serious and safe work in anthropology, or even in certain lines of antiquarian research, can possibly be accomplished in these days without the assistance of one or more experts in psychology. No anthropological commission should be sent out by either the Institution or the Government of the United States unaccompanied by such an

expert. The delicacy of making any device and the scandal of making a mistaken choice, and the almost greater scandal of trying to advance anthropological researches in neglect of the work of the psychologist, are additional reasons for the Institution to lend a strong, helpful hand here. The same thing is true if we are ever to have any worthy results from the experimental study of our most fundamental and pressing problems of the education of our public schools. Again, in many cases of more private researches the handling of the data obtained by some expert computer or their review by some one skilled in psychological tests and measurements is necessary to determine their value.

4. In the fourth grade of importance, if not even higher up in the scale, should be placed the aiding of individuals in researches where the expenditure of time, talent, and money makes it difficult or impossible for such individuals to conduct, without aid from the Institution, their researches successfully. These grants to individuals should, of course, be made very judiciously, and even sparingly, as respects numbers. They can probably never bear any large proportion to the number of requests for grants; but in certain cases they will need to be generous in order to be effective; and it must be borne in mind that the true investigator does not always know, by any means, just where he is coming out or what point his investigation may produce.

5. I do not favor inaugurating at present any system of fellowships. In my judgment, the entire business of fellowships has been quite overdone by our universities. In certain rather rare cases very promising young men might perhaps be sustained in their researches, either wholly or in part, by the Carnegie Institution. In general, however, only a percentage of those now enjoying such assistance are really worthy of encouragement for the higher purposes of even university work. From the ranks of some of our younger *teachers* I should think occasional Fellows of the Carnegie Institution might be temporarily drawn off.

6. Later on, and after these more immediate needs are met, the Institution might profitably use its large resources in fostering the study of animal and comparative psychology.

7. At the same time, or perhaps earlier, if funds are available, and, what is harder to secure, men who are competent, work in pathological psychology and in the investigation of defective children, etc., might also be undertaken.

In both these lines of work I think that the movements of the

Institution should be very slow and cautious, except in so far as they can assist in such work as it falls under the heads of the proper functions of the Institution, as enumerated above.

8. Printing and publishing the results of research and the reports of commissions and committees should, I think, be done by the press of the Carnegie Institution, if it is deemed wise to establish such a press. I should favor its establishment.

9. Occasionally, perhaps, but only very rarely, should the Institution assist individuals in publishing books which are not prepared as part of the Institution's work.

10. I do not favor the plan of having the Carnegie Institution compile or assist in compiling a bibliography of psychology.

In closing I wish, as the head of the department of psychology in Yale University and in behalf of my colleagues, to express our grateful appreciation of the magnificent gift of the founder of the Carnegie Institution, our hearty sympathy with his general purposes, and our willingness to coöperate in carrying to success these purposes, so far as fidelity to the university renders such coöperation possible.

[*Dr. Hugo Münsterberg, Professor of Psychology, Harvard University, to Mr. Baldwin.*]

PROPOSITIONS FOR THE USE OF THE CARNEGIE FUNDS IN THE INTEREST OF PSYCHOLOGY.

(Nos. 1–7 to be Established in Washington.)

Most Valuable.—1. A subsidized printing establishment for monographs in psychology and other sciences, which would print and prepare plates on a commercial basis, for any customer, at a rate comparable to that ruling in Germany or France.

2. A subsidized mechanical establishment for the construction of psychological and other instruments at European rates of expense.

Very Valuable.—3. An institution for psychological experiments on men, with special emphasis on such problems as can not be easily studied in the usual university laboratories. Here belong—

(*a*) Experiments on the influence of abnormal conditions which as such are undesirable in an educational institution.

(*b*) Experiments which demand more time than the university students can afford to give.

(*c*) Experiments which need a more elaborate equipment than the universities can afford.

(*d*) Experiments which demand a large number of subjects. Necessarily such an institution would not only appoint conductors to assign problems and methods, but also award fellowships to highly advanced young scholars, who could serve as self-observing subjects of the investigations. In this respect such an institution would be different from a physical or chemical institute, and distinct also from the two following.

4. An institution for animal psychology. Here belong—

(*a*) Experiments on such animals as by their size or habits of life cannot be kept in the regular university laboratories.

(*b*) Studies in heredity.

(*c*) Experiments on animals under abnormal conditions.

Valuable.—5. An institution for anthropological-psychological measurements and statistics.

6. A bureau for the exchange of psychological observations in the laboratories of the country (provided that no attempt is made to control the various laboratories), for computing results which any psychologist may send, for compiling literature, for giving information on literature to psychological investigators. etc.

7. An institution for pathological psychology, working in connection with an asylum.

Valuable but Very Dangerous for the Development of the Educational Life of the Whole Country.—8. Aid to individual scholars who are acknowledged to be specialists of merit, in the form of aid for instruments, assistance, printing expenses, leave of absence—all this on application of the scholar.

9. Aid for young, promising doctors of philosophy, in the form of fellowships for work to be done independently of the institutions to be created in Washington; such aid to be given merely on application from the academic teachers of the recipients.

[*Dr. James McKeen Cattell, Professor of Psychology, Columbia University, to Mr. Baldwin.*]

If the Carnegie Institution establishes laboratories in Washington, I should place first a psychological laboratory for the investigation of problems requiring exact methods. This laboratory might supervise—

(*a*) Printing press and instrument shop in connection with the other sciences.

(*b*) Bureau of computations in connection with the other sciences.

(*c*) An anthropometric institute in connection with anthropology.

(*d*) An institution for tests, etc., in schools, in conjunction with the Bureau of Education.

(*e*) An institution for work on the defective classes, in conjunction with asylums, etc.

(*f*) An institution for psycho-physiology in conjunction with work in physiology.

(*g*) An institution for comparative work in conjunction with biology and a zoölogical park.

(*h*) A pathological institution in conjunction with work in psychiatry.

(*i*) Bibliographic work and a library.

This is in general the order of my preference, though I think that the direction of work should depend chiefly on the men and facilities at hand. I can not well place in order of merit the ways in which the Institution can help psychology throughout the country. I have discussed the matter in detail in *Science* and can not give a concise list without danger of misunderstanding.

[*Dr. Joseph Jastrow, Professor of Psychology, University of Wisconsin, to Mr. Baldwin.*]

1. I am of the opinion that the most decided aid would come from the establishment of a research laboratory in psychology, or, next to that, of research endowments for existing laboratories in general accordance with my suggestions in *Science*. While there are many psychological projects which it is of great importance to further, it is, in my opinion, still more important to attract the energies of psychologists into the research field by establishing positions in which capacity for research is the main requirement, and by transforming the status of their present positions so as to make possible an emphasis of the research side of their profession. My first answer would then be to aid research in psychology by supplying personal assistance to men who have the promise of doing something noteworthy, giving grants for clerical aid, for computers, for statistical expansion of investigation. Aid men so that the time they do spend on research shall be most efficiently spent. The endowment of specific projects, though important, is to me not so important as this more difficult, but in the end more efficient, stimulus.

2. I am willing to express my opinion that there is room in Washington for a psychological position of wide influence. The Bureau

of Education distinctly needs the services of a psychologist. In certain aspects of the work of the Bureau of Ethnology, of the Surgeon General's Library, of the Smithsonian Institution, and of the National Museum, the coöperation of a trained psychologist would be distinctly helpful. It should likewise be borne in mind that there are in Washington and its vicinity educational institutions furnishing opportunity for the engagement of a psychologist of high standing in advanced instructional work.

3. Of specific problems worthy of encouragement I bring forward a few which have engaged my special attention :

(*a*) A commission for the invention, examination, and establishment of mental tests and the dissemination of the laws, so that the normal endowment of man in regard to his fundamental psychological equipment may be determined An important factor of the problem consists in the investigation of the correlation of such functional efficiency with developmental changes. Such *normals* would find further application in the study of abnormal variations; in the determination of the correlation of the several avenues and types of mental endowment with one another and with physical capacities; in the practical application to educational methods, and in other directions less readily specified.

(*b*) A special study of the psychological status of the processes most fundamental to elementary education. Speaking, reading, and writing form one group of these; the associative processes of mathematical calculation and logical inference form another. The object is to furnish a positive basis upon which education may rely for deciding between rival methods and to terminate the endless and haphazard discussions that form so large a part of educational diversities of opinion.

(*c*) The provision for the psychological study of abnormal mental traits, both in connection with insane asylums, the institutions for the defective classes, the examination of special individuals, etc. The purpose of this would be to give a more adequate formulation to the psychological side of the care and treatment of the mentally defective and to furnish the means by which the many false and misleading notions and observations now current in regard to such extreme mental variations might be more successfully combated. Psychic epidemics and popular movements of a promiscuous character can be opposed most effectively by providing an authoritative statement, to which those who instruct the public could readily turn.

(*d*) A station for the strictly psychological study of animal intelligence is of critical importance for the further advance of comparative psychology.

I have confined my enumeration to a few of the projects of larger scope which a central establishment for research could most suitably direct, and which are less likely than others to be taken in hand by existing facilities for research.

4. Of coöperative measures I should regard as most helpful—

(*a*) A central scientific instrument works, in which research apparatus could be brought to perfection, and from which standard apparatus could be issued.

(*b*) The endowment of bibliographical aids to the student of psychology, and the provision of adequate representation of psychology in more general bibliographies.

(*c*) The provision for the representation of psychology in anthropological expeditions, so that the mental status of the various races may be established. Such opportunity is in many cases likely to disappear, owing to the commingling and extinction of peoples.

(*d*) The establishment of branch depositories for the gathering of tests of normal mental efficiency in connection with the project outlined in 3 (*a*).

[*Dr. Edward Bradford Titchener, Professor of Psychology, Cornell University, to Mr. Baldwin.*]

1. On the assumption that psychology is to benefit by the Carnegie fund, the question at once arises whether the sums allotted to psychology should be spent in a lump, for one purpose, or whether they should be distributed for various ends. As regards psychology itself, the question arises whether we are concerned to further the immediate needs of the science, to hasten progress along lines already laid down, or whether we desire to erect a permanent monument, which may lie a little outside of our visible needs, but which will be of enduring value, whether psychology advances along its present track or undergoes such another revolution as it suffered with the introduction of the experimental method.

On the former issue, my own opinion is that we should either effect a compromise or boldly put the whole fund into commission. I am not in favor of a single lump-sum expenditure. On the latter I think that the immediate needs of psychology are pressing; that we now require, not a few geniuses, but rather a large body of

capable, rank-and-file workers, who shall thoroughly work out the multitude of problems handed down to us from the second half of the nineteenth century; that the work before us, on lines already laid down, will occupy at least two human generations, and that it would be unwise to allow for or to try to envisage the progress of psychology beyond this point.

These matters are, however, extremely debatable. Fortunately, the present conditions of psychology practically dictate to us, irrespective of our personal beliefs, what is the first thing to be done with a Carnegie grant.

2. We must, in the first place, have a share in a subsidized establishment for printing, engraving, lithography, etc. There should be no interference with the integrity of existing journals; but we ought to be able to have articles prepared for publication, on the mechanical side, as cheaply as is possible on the continent of Europe. The establishment should have skilled proofreaders, such as are attached to the larger German printing houses—technically trained readers. I have not myself felt the need of trained computers and statisticians, such as are required in some forms of genetic work and in work on mental tests, but I see no reason why this same establishment should not have a staff of computers also. The need of them is certainly pressing in some departments of psychology. At any rate, the need of printers and engravers is imperatively pressing in all departments.

I am, then, entirely clear on the point that, whether we are to have one thing or many, we must have this. It is now hardly conceivable that our share in the establishment will exhaust our share of the funds. What is to be done with the rest?

3. If the funds admit, I should here make the compromise spoken of just now. I should sink all the rest of the money in a central psychological institution; but it must be understood that this institution is not to duplicate, or slightly to improve upon, existing laboratories. It must be an *over-laboratory*. It must be able to perform work which for any sort of reason—time, expense, difficulty, number of observers, necessity of pathological material—can not be performed in a regular university laboratory. It must make full and adequate provision for work in comparative psychology, perhaps by granting the use of zoölogical collections, perhaps by way of an experiment farm, perhaps by arrangements with existing biological laboratories, or by all of these means together. It must be representative of the whole of psychology; which implies that, while it

is to have a permanent administrative staff, its direction must change, on the scientific side, every three or five or so many years; no single man is nowadays representative of the science. It must have inducements to brilliant young men, in the way of fellowships given for three or five or some good round term of years—competitive fellowships, strictly limited in number. It must have a full collection of historical instruments; a museum, with descriptive labels and references, always open to investigators from elsewhere; perhaps a loan collection of elaborate pieces; perhaps a workshop, where instruments could be procured by the university laboratories at cost. To be complete, it should have a library and a bibliographical establishment; though I regard these two items, under existing conditions, as of minor importance.

Such an institution would be both of immediate and of permanent value to psychology. I greatly doubt, however, whether the idea can be realized. There are many sciences, and the Carnegie fund is limited. Much saving might be effected by affiliation with existing zoölogical gardens, insane asylums, biological laboratories, etc.; but I should regard the narrowing of the material equipment of the Institution—anything that tended to make it a name or a bureau—and the scattering of the men connected with it as exceedingly dangerous. To do the work which I have in mind, the Institution should be as imposing materially, by its block of buildings and its centralized staff, as morally by its purpose and program. It should be, literally, a central station for psychology. While availing itself of local opportunities all over the country, it should bring together, for a part of each year, the best men in all departments of psychological inquiry. It should be a visible witness to the range and diversity of psychological problems and interests. Anything in the nature of a halfway house or a first beginning I should look on with grave suspicion.

If, as I suspect, there is no prospect of realizing such an institution as I have here sketched, I should recommend the use of the Carnegie grant for the purposes named in my *Science* paper, namely:

4. Valuable fellowships, of $750 or $1,000 for two or three years, granted to doctors of philosophy of acknowledged power and merit; these fellowships to be held at existing institutions, at the choice of the appointees; living wages, of $300 or $400 for one year, granted to promising graduate students who are too poor to pay their own way; grants of $500 to $1,000, made to professional psychologists, without demand of program or promise of result, on their personal

guarantee (backed, perhaps, by a committee of their colleagues) to expend the money for the advancement of science in some fairly definite direction. I propose these things as an alternative to the creation of the central institution. They do away with any need of or excuse for an expensive central establishment in Washington. I should prefer the institution. At the same time, I do not see any general danger to the cause of education in the creation of these three forms of subsidy, granted practically to existing universities.

The list of *Projects in Psychology* contains twelve sections.

No. 11, a printing establishment, I put first.

Nos. 1, 2, 4, 5, 6, 7, 8, 10 I have dealt with, in order of their apparent merit, under my discussion of the central institution.

No. 9 is covered by my alternative proposition to that of a central institution.

Nos. 3 and 12 appear to me, however valuable in themselves, to lie outside of psychology proper.

REPORT OF ADVISORY COMMITTEE ON HISTORY

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: The undersigned, requested by you to serve as an advisory committee in matters of history, beg leave to submit the following report, based upon a careful consideration of the present status of historical studies in the United States:

Introductory.—While we think it probable that ultimately considerable sums may well be expended, under adequate methods of supervision, in directly aiding the work of individual investigators along special lines marked out by themselves, we make no recommendations of that kind at present. We believe that results permanently and widely valuable to historical science are much more likely to be obtained by devoting attention first of all to the promotion of general and comprehensive yet definite projects, helpful to the profession at large. We should give the foremost place to certain tasks naturally fundamental, tasks which logically come first as the necessary preliminaries to generations of successful individual work. It seems to us clear that, for some years at least, attention should be mainly concentrated upon American history, European history being relatively well cared for already by existing European agencies. We also believe that it is well not to make initial recommendations or arrangements too elaborate, experience showing that it is wiser to give scientific establishments an opportunity to grow in the hands of those who direct them.

So far as we can judge from such consultations as we have been able to have with historical students, the principles above stated seem to have the general approval of the profession.

On these general principles we make three recommendations:

1. *An Institute of Historical Research in Washington.*—More American historical work of wide utility can be done in Washington than anywhere else. It is desirable to found there a permanent establishment under a competent director, who should be given some freedom to develop it. There are goods models, *mutatis mutandis*, in the French, Prussian, and Austrian institutes at Rome. Such an establishment could serve four important purposes:

(*a*) *First* of all, in logical order, it should execute a comprehensive and detailed examination of the Government archives, resulting in

the preparation of a monumental report upon the vast store of manuscript materials for American history now preserved in Washington.

(*b*) *Secondly*, it might thereafter proceed, upon a carefully considered plan, to the scholarly editing of whatever might seem most important in this mass of historical material. If a bureau maintained by the Carnegie Institution (in which Congress will, no doubt, have the same kind of confidence as in the Smithsonian) should undertake to provide such bodies of documentary material, prepared in the best manner for publication, there can be little doubt that Congress could readily be persuaded to provide for their printing. The result might be a noble series, comparable to the *Monumenta Germaniæ Historica* and other collections of sources achieved in Europe by the mutual coöperation of governments and scholars.

(*c*) *Thirdly*, such an institute should serve as a clearing house for the historical scholars of the country. It should facilitate their personal researches in Washington and, so far as possible, aid those who are at a distance to avail themselves of its treasures.

(*d*) *Fourthly*, it should provide suitable guidance and instruction for such advanced and highly competent graduate students as should resort to it for the purpose. The number should be kept small by high requirements. But it would be of great advantage to the future of historical instruction and investigation in this country if every year a few of the best and most advanced graduate students of history could, before they begin their life work, be properly introduced to the rich stores of historical material in Washington and to its inspiring atmosphere, and, it should be remarked, there exists now no regular means toward this end, such as the scientific establishments of the Government afford to students of the physical sciences. In such instruction the director should have the aid, perhaps by annual rotation, of a succession of professors from the leading universities.

To proceed cautiously and to make a right beginning are so important that it might probably be best that the director chosen for such an institute should, for the first year, or at least for some months, occupy himself solely with such inquiries, examinations of archives, and consultations in Washington and elsewhere as should enable him to devise judicious detailed plans for its organization and early operations.

We judge that to obtain the service of the best man for such a position the Carnegie Institution should be prepared to pay a salary of $5,000 per annum; that $3,000 more should be appropriated to pay each year the professor who should teach on leave of absence from his university; that the assistance of younger men and of a typewriter would be needed in the work of the institute to an extent represented by about $2,000 a year, and that there would be need of an office, a small seminary room, and a small library of historical books of reference. An annual expense of $12,000 might be reckoned upon as adequate; less would suffice in the first year.

2. *A Search of European Archives.*—Vast masses of material for American history exist in European archives. Incomplete examinations of them, partial reports upon them, have abounded, and scholars have used them, but for the most part casually and without the possibility of being certain that their searches were exhaustive. Good logic, good sense, and the example of the best European practice would alike dictate that inventories should come first, and exploitation afterward; that we should first find out what material there is, and then lay plans for using it. We therefore recommend that steps be taken toward a thorough examination of the archives of Europe, with a view to comprehensive and detailed inventories of the materials which they possess for the history of the United States and its various parts and dependencies. It should include a search of national, and in some cases of provincial, municipal and family archives; of the archives of the Vatican; of the great religious orders; and of other ecclesiastical bodies and officials formerly holding sway in any part of America. Especial attention should be paid to the repositories of historical manuscripts in Spain relating not only to the history of the United States, but to that of our new possessions, which can not be properly managed without a completer knowledge of their previous development.

There are two possible ways by which such an inquest might be conducted. It is possible that a competent supervisor might be engaged to reside in Europe continuously until the inquiry is finished, to employ proper persons to make searches simultaneously in each country, and to push the task to a speedy conclusion. But practical difficulties of detail might work against this plan. The Americans best fitted to search in various countries respectively, though usually able to secure an occasional year in Europe, might not be able to be abroad simultaneously. In that case another plan would be preferable, though slower in covering all Europe. According to this,

without employing a general organizer for all Europe, the problem could be taken up country by country. The man most competent to search for and inventory American materials in Italy, for example, having been selected, he could be sent there the first year in which he was free to go. The searcher should, of course, be provided with proper assistants, manual and scholarly. It is possible that, as in Washington, some of the features of a training school could be combined with the work of exploration. The searchers might take with them properly qualified graduate students and use them as apprentices.

But the countries of Europe differ so widely one from another in the profusion, character, and accessibility of their materials for American history, and in the extent to which these have been catalogued and used, that we do not believe it possible at this distance and in the present state of our knowledge to decide off hand upon that plan of operation in the examining of archives which shall in each country lead to the completest information and the most useful form of inventory. We therefore recommend as the first year's work in this field that as soon as it is practicable the Trustees of the Carnegie Institution engage for one year the services of an accomplished American historical scholar, already well acquainted with several European archives, who shall visit the various states of Europe, collect information concerning conditions, persons, and methods, and report a comprehensive plan, adapted to the circumstances of different countries, for the conduct of the detailed inquest above described. He should have proper clerical assistance. We suggest an appropriation of $5,500 for this preliminary survey—$4,000 for salary and traveling expenses and $1,500 for clerical assistance. To what extent and in what directions the work in Europe should ultimately grow and what its future needs would be we do not now attempt to prescribe.

3. *The American Historical Review*.—The *American Historical Review* is at the present time the chief means for the publication of brief historical researches. As such it is doing a great service to American historical scholarship. It is, in a quite exceptional sense, the constituted organ of the profession. It was founded by a union of all historical interests. It is owned and managed by a board whose members are professors in various universities, and who are elected by the Council of the American Historical Association. That Association, without seeking to influence the policy of the *Review*, gives it an annual grant as large as it can afford. With

this aid it has barely succeeded in paying its expenses. But it has done so only by using a guarantee fund raised at its inception and now nearly exhausted, and by reason of the fact that, while it pays three dollars a page (and the book) for reviews of books, it pays nothing for "body articles." Its quality as an organ of American historical scholarship would in our judgment be greatly raised, with valuable results to scholarship itself, if it could also pay four dollars a page for articles. This would enable it to command articles from the best specialists or the leading professors of history. These are often, instead, writing for journals that pay, but would rather write the kind of articles which the *Review* desires, the fruit of solid researches scientifically presented, if they could get something for them, though it were less than the popular magazines pay. To enable this to be done, and to avoid the deficits hitherto met by the use of the guarantee fund, we recommend a subvention of $2,000 per annum to the *American Historical Review*.

If such a relation were established with the *Review*, the historical department of the Carnegie Institution could undoubtedly use a section of the journal as a means of direct and regular communication with the historical students of the country. Whatever modes of publication the Carnegie Institution might adopt for its general purposes, it would be a distinct advantage to have quarterly bulletins concerning the work of its Institute of Historical Research in Washington, or concerning the progress of its work in Europe, inserted in a journal which goes to all members of the American Historical Association and is read by all historical scholars in the United States. Constant interest of the special public in its historical activities would thus be insured.

There are several other objects of historical expenditure which appeal strongly to us, such as the work of the Historical Manuscripts Commission and the Public Archives Commission established by the American Historical Association, and the necessary researches for a scholarly atlas of American Historical Geography or a Dictionary of National Biography comparable with the English. But these either are going slowly forward, with money supplied by the Association, or can wait. We conclude to emphasize, as of primary importance to American historical scholarship at the present time, only the three objects we have described above.

The appropriations which we recommend are: For the Institute of Historical Research in Washington, $5,000 the first year, $12,000 thereafter (or from the completion of the director's plans for organ-

ization, which might be completed in less than a year); toward the preliminary search of European archives, $5,500 in the first year; for the *American Historical Review*, $2,000 per annum.

Respectfully submitted by

J. FRANKLIN JAMESON, *Chairman*,
CHARLES FRANCIS ADAMS,
A. C. MCLAUGHLIN,
Committee.

NEW YORK, *October* 25, 1902.

REPORT OF ADVISORY COMMITTEE ON MATHEMATICS

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: Your Advisory Committee on Mathematics begs leave to submit the following report:

General Statement.—Mathematics, though abstract and bearing the closest relations to pure logic, is most intimately connected with all the physical sciences, and through the rise of the statistical method is becoming of increasing utility in the other sciences. In this country mathematical research is thoroughly alive and full of promise, especially along various lines of pure mathematics and celestial mechanics. As to the future, it is certainly desirable that more men, with full appreciation of modern mathematical principles and processes, should devote themselves to investigation in the various natural sciences.

In the applications of mathematics, statistical methods stand at present in the foreground. They would seem to have a future in the problems of meteorology, biology, and other branches of science. Their natural use arises in connection with attempts to put theories such as Darwin's to a direct test, and in investigating possible inter-relations between one set of physical data and another set. Such inquiries, to have value, require the coöperation of mathematicians who know exactly the true relation of the mathematical machinery and of experts in the particular science; for the mathematician by himself lacks the quasi-instinctive recognition of absurdity in erroneous results, and can not estimate the value of the data; and, on the other hand, the expert by himself is apt to regard conclusions as generally true which are based to some extent on assumptions introduced to simplify the mathematics.

Thus, for instance, in meteorology and terrestrial magnetism there are enormous accumulations of data, and the investigation as to the existence of certain cycles and the sequence of weather types is of obvious importance. Thus there is a wide field for statistical investigation of a kind that calls for skilled mathematical power, as well as special insight in the subject matter; and the same is true, no doubt, in biology and anthropology and economics.

These considerations point to a Bureau of Statistics.

The American Mathematical Society.—The mathematical interests of this country are already fortunate in that they have secured or-

ganic expression in the American Mathematical Society. This society, organized in 1889 as the New York Mathematical Society, rapidly became in fact a national society, and was recognized in 1893 as the American Mathematical Society. It embraces in its membership, almost without exception, every working mathematician in this country and also a number of European mathematicians. The policy of its management has been one of consideration for all phases of mathematical activity. Its *Bulletin* was published from the beginning as a monthly journal of historical and critical character. In 1900 publication of the *Transactions* of the society was begun, a group of ten universities temporarily supporting the society in the financial burden. With the beginning of the publication of the Transactions the society has undertaken to complete its library and transform it into a valuable circulating library, available for the use of its members and all other mathematicians.

The activities of the society are set forth in more detail in the subjoined letter from its secretary. In our judgment, any additional income which may come to the American Mathematical Society by way of subvention from the Carnegie Institution will be well used by the society to promote mathematical research in America. We recommend in particular that for a term of years $1,000 annually be granted to the society for the purpose of converting its library into a thoroughly good collection of books and models, to be placed unreservedly at the service of the working mathematicians of the country.

American Journals of Research in Mathematics.—Many leading mathematical journals of Europe receive subventions from government sources. We recommend that there be granted subventions to the following journals of this country:

	Per annum
American Journal of Mathematics	$1,000
Transactions of the American Mathematical Society	1,000
Bulletin of the American Mathematical Society	500
Annals of Mathematics	500

These four journals are most powerfully promoting the interests of mathematics in this country, and they would be enabled to become still more valuable factors by the receipt of such subventions.

The very considerable labor of editing these journals, as things stand, falls upon men who as leaders in research have become prominent in the great universities—that is, upon men whose energies are already heavily taxed.

The ideal editor of a scientific journal should exert, especially upon the contributors just entering upon their career of research, an extremely valuable educational influence. Hence, if possible, he should be set free for that work. To accomplish this he should receive all the assistance possible that can be performed by a younger mathematician in a clerical way, such as expert proof reading, etc. Furthermore, as a result of the rapid development of mathematics in so many different directions, it is impossible for any one editor satisfactorily to judge of the merits of the manuscripts sent to him, and he is compelled to seek aid from specialists of established reputation as referees. The work of these experts requires time and energy, which it is unjust to demand without payment therefor. The present limited incomes of the journals preclude such payment, and thus there result much delay and other loss of efficiency.

Treatises of Advanced Character and Collected Works.—The European academies arrange to publish the collected works of distinguished scientists. In England the great university presses perform the same function, and, furthermore, facilitate the publication, even at a financial loss, of treatises of high scientific character. It seems fitting that the Carnegie Institution should perform a like function in this country.

We earnestly recommend that the Carnegie Institution arrange for the collection and publication of the works of George W. Hill. We estimate the expense at $2,000 per year for six years.

We furthermore recommend the publication of the collected works of Euler, which would constitute a thesaurus of the mathematics of the eighteenth century. The expense is estimated at $3,000 per year for twelve years.

Grants to Individuals in Aid of Research in Mathematics.—It should be understood that applications may be made to the Carnegie Institution for aid in specific researches; for example, in the preparation of tables or of bibliographies or of reports, and in the construction of new models or mechanisms; but that applications for aid in study without defined productive purpose are not desired.

A most obvious way of promoting research would be to give more freedom or facilities for research to those men or groups of men who through recognized original power are professors in the great universities. One wise form of aid would seem to lie in attaching especially able students who have taken the doctor's degree to such leaders in original research as *research assistants*, an arrangement serving at

the same time to increase the activity of the older men and to establish in the younger men a habit of research.

We set down in conclusion a *summary of the specific recommendations of this report :*

Subventions in aid of publication : Collected works—G. W. Hill, for six years, $2,000 a year ; Euler, for twelve years, $3,000 a year.

Mathematical journals of reseach as listed above, $3,000 a year.

Subventions in aid of research : Toward the establishment of a lending library of books and models, $1,000 a year.

Respectfully submitted.

E. H. MOORE, *Chairman*,
FRANK MORLEY,
ORMOND STONE,
Committee.

NOVEMBER 5, 1902.

[*Letter from F. N. Cole, Secretary American Mathematical Society, to Prof. Morley.*]

501 WEST 116TH STREET, NEW YORK,
August 28, 1902.

MY DEAR PROFESSOR MORLEY :

In reply to your recent letter in which you suggest an expression of my views in regard to the distribution of that portion of the Carnegie fund which may be assigned to mathematics, I should like to present the following considerations :

Assuming that other mathematical interests of the country will receive deserved recognition, I shall confine myself to a presentation of the claims of the American Mathematical Society, and the discussion of the manner in which any funds which it may receive may be most efficiently employed.

If the Carnegie fund is intended to help those who can help themselves, then the record of this society entitles it to most liberal recognition. For fourteen years it has been the center of mathematical activity in this country. Including in its membership nearly every American mathematician of any standing, it represents their organized strength and interests. It has stimulated their great advance and been stimulated in return until its overwhelming prosperity taxes its administrative resources to the utmost. It publishes two

journals which barely provide for its present scientific output. It has also published the first volume of a contemplated series of mathematical papers. The editors, the librarian, and the secretary carry on a correspondence equal to that of a large business house. The meetings nine each year, are largely attended, and every minute is utilized for improvement.

The society has expended from the pockets of its members, in the past eleven years, abont $15,000 for the advancement of mathematics, of which $5,000 has been spent in the last two years. We carry a balance of $2,000, of which about $900 has been reserved for a special fund ultimately for prizes or special publications, etc.; but this balance can not long be maintained.

Of all the money expended, not a cent has gone for salaries. The officers and editors all serve without the slightest pecuniary compensation; and in several cases their work is very burdensome and involves minor as well as major administration, for the officer has almost always been his own clerk, in order to avoid an expense which the society's fund did not permit. The services which the society annually receives gratis would hardly be fairly paid for by its entire income, if one may compute them in money at all.

Our membership is now 400. Our income last year from members' dues was $1,188; from sales of publications, etc., $859; from ten universities in support of the transactions, $1,000; total income. $3,747: expenditure, $3,772. We need $1,500 a year more in order that the energies of the society may be turned to full account for the advancement of mathematics in America.

Considering the representative character of the society, the skill with which it has been administered, the unselfish devotion of its administration corps, and its knowledge of needs and how to meet them, I think that the Trustees of the Carnegie fund might well feel that they could grant to the society a lump sum from year to year, to be employed as the Council of the Society might determine, accounts of course being rendered to the Trustees, who would control the situation perfectly, since they could refuse further appropriation at the end of any year. My point is that the Mathematical Society is in a position to administer any fund granted to it, in the best possible way, and its history is evidence that it will not fail to do so.

But if it is decided to make specific appropriation, then I would submit the following synopsis of the society's various activities which would be promoted by subvention:

First. The society's publications, viz., the *Bulletin* and the *Transactions*, with possibly an occasional separate publication. All these can turn money to good account. Some enlargement is already desirable, in view of the increasing number of valuable articles offered for publication.

Speaking for the *Bulletin*, I must say that the expense of editing, which was last year $33.94, can not longer be kept down to any such vanishing point. The editors can not afford to attend to all the mechanical details at the expense of the greater interests of the *Bulletin*.

Second. The library of the society is now growing rapidly. It is becoming a credit to the society, and, like all healthy institutions, is in need of money. The files should be extended and the back numbers purchased when they can not be got by exchange. We need $100 a year for this purpose, and can use $500.

Third. We have had three successive colloquia in connection with our summer meetings. These are courses of lectures by specialists, and tend most pronouncedly to the early dissemination among the mathematicians of the country of important mathematical advances and discoveries. The lecturers have received only a trifling honorarium. For the promotion of mathematics at the very top, nothing could be more effective than the foundation of one or two lectureships, to be held for one year, by incumbents who are able to treat the very latest phases of some branch of mathematics, the lecture to be arranged as heretofore at the colloquia. This would involve an expense of, say, $100 to $200 a year—*i. e.*, $200 to $400 every alternate year.

Fourth. In order that administrative officers may attend to pressing matters of larger policy, a moderate amount should be available for clerk hire. The librarian and the secretary can not much longer look after the multitude of petty mechanical details which the administration of so large and active a society as ours involves. Mr. Carnegie did not conduct his business on any such plan.

Fifth. Those specialists who pass on the suitability of papers for publication, particularly in the *Transactions*, should receive a reasonable compensation, or, rather, *honorarium*, for it need not be any large amount. It will not be always possible to receive satisfactory expert advice gratis, and this very advice is what must decide the character, good or bad, of the publishing journal. I regard a moderate expenditure in this direction as a good business and scientific policy.

There are other interests, and some interests vary and alternate from year to year. As I have said, I regard the society itself as the best judge of their merits, and I would much prefer that it should be the distributing medium. It would be fortunate, for example, if any one interest should be so preferred by the Carnegie Trustees that the judgment of the Council should not approve of the conditions. In my opinion, the Council should appoint a committee to look over the field and report, and should then present its views to your Committee for mutual discussion, the conclusion being in the form of a recommendation by your Committee to the Carnegie Trustees.

The above is a general expression of my personal views. I have endeavored to be brief, and possibly I should have recited some matters more at length; but as you are a member of the Committee of Publication and familiar with the affairs of the society, the above will probably suffice to suggest the details which I have omitted.

Sincerely yours,

F. N. Cole.

APPENDIX B

PROPOSED EXPLORATIONS AND INVESTIGATIONS ON A LARGE SCALE

CONTENTS

INTRODUCTION.

Among the applications and projects received by the Institution were several of large scope that have not been mentioned in the reports of the Advisory Committees, printed in Appendix A. There are others not yet considered that may be referred to in a future report. The Advisory Committees reported them to be of great interest and importance, but deemed it unnecessary to say more until the plans and policy of the Institution had been further developed and determined. Nine of the more important projects are given here.

PLAN FOR A BIOLOGICAL SURVEY OF THE PALEARCTIC REGION

By LEONHARD STEJNEGER AND GERRIT S. MILLER, JR.

CONTENTS

WASHINGTON, D. C., *May 19, 1902.*

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: In response to the invitation of the Carnegie Institution through its circular received in March, 1902, the undersigned would respectfully submit for the consideration of the Executive Committee the following plan for a Biological Survey of that portion of the Old World lying north of the tropics, and technically known as the Palearctic Region. An outline of the magnitude, variety, and importance of the biological problems on which this work would bear is presented in the pages which follow. To this is added a detailed plan for the organization of the work, together with an estimate of its cost. This enterprise, if undertaken, would bear a close supplemental relationship to the similar work which has been carried on with such eminent success during the past fifteen years in America under the auspices of the United States Government, and would furthermore be the direct continuation of studies which have largely occupied the attention of one of the writers for eight years and of the other for more than three times that period.

Very respectfully,

LEONHARD STEJNEGER.
GERRIT S. MILLER, JR.

I. INTRODUCTION.

1. *Nature of Work Proposed.*—Briefly stated, the proposed investigations consist in the thorough exploration of the northern portion of the Old World by parties of competent, trained field naturalists, with the special objects of determining in detail the character of the fauna and flora, the number and geographic extent of the life areas, the history of the origin of the present assemblages of animals and plants, and, finally, the relationship of the present life of the region to that of northern America. That these large, fundamental problems are now vaguely or not at all understood will be shown in the special section of this report devoted to them.

As the material for the elucidation of these problems does not exist in any museums or museum, a very important part of the work would be the bringing together of specimens collected by the field parties, and their study at home by specialists. That the extent and value of these collections would be unprecedented there can be no doubt. It would be proposed to make as complete collections as possible of the mammals, birds, reptiles, batrachians, and fresh water

fishes, together with such insects, mollusks, and plants as may be found to have special bearing on the work. It is hardly necessary to add that extensive publications would undoubtedly result from the elaboration of the collections

As to the length of time required for this work, it is difficult to make any close estimate. If carried out on the scale here proposed, the field explorations would probably be fairly complete in ten years.

2. *Reasons Why it Should Be Undertaken by the Carnegie Institution.*—For generations zoölogists and botanists have been working piecemeal at these problems, and have expended a vast amount of labor and learning, yet it is a deplorable fact that no adequate results have been achieved. It would carry us too far were we to discuss the reasons why the Old World workers have been unable to gather the necessary material and work it up in a harmonious, comprehensive manner, so as to make the results available to others for purposes of coördination and generalization. They are of a historical, political, and financial nature. In the meantime our own countrymen have advanced so far in these particular respects—thanks to well planned, well executed, and comprehensive work by the U. S. Biological Survey—that the inaccessibility of the corresponding facts relating to the fauna and flora of the Old World has become a formidable obstacle to our own progress.

Unfortunately, the conditions which in the Old World have contributed to bring about this situation are permanent, so that no relief can be hoped for from that direction. Moreover, no Old World museum, university, or academy could undertake the work, even if so inclined and with the money, because it would be practically impossible for them to duplicate the work already so thoroughly done on this hemisphere. If done at all, the work must be undertaken from this side. We are already far ahead in the survey of our field, and the Old World material must be gathered by us, with our methods to make it comparable, worked up by our scientists to insure its uniform elaboration, and deposited in our institutions to accomplish a really valuable correlation; but thus far we have labored under the same difficulties as the Old World biologists. No institution in our country has been so situated scientifically and financially that it could think of realizing so comprehensive a scheme; nor can the Government be expected to extend its activities into this field on a scale which would promise results within a measurable future.

Other problems, such as the investigation of the more truly American fauna and flora to the south of us, as well as the biolog-

ical exploration of the vast territories recently acquired by the United States, are more likely to occupy the attention and energies of our governmental surveys.

Under these circumstances the Carnegie Institution, untrammeled by political boundaries and in possession of sufficient means, seems to be the proper agency for carrying out this important enterprise.

In brief, our contention may be summed as follows:

The proposed scheme would be of great value to biologic science.

The need for undertaking the work now is urgent.

The plan is practicable; it is no experiment: its promoters have largely devoted themselves to it; the methods are perfected; the time is ripe; the cost is moderate: no other institution can now undertake it.

The Carnegie Institution must therefore take it up if the important scientific questions involved are to be satisfactorily solved in the present generation.

3. *Feasibility of the Work.*—Geological exploration on a large scale and with a definite object in view has long been recognized as a profitable scientific enterprise. The importance of the results achieved by such investigations are so well known that they need no more than this passing allusion. In the same way extensive, specially equipped expeditions for investigating the life of the deep sea bottom are almost continually at work; vast sums are annually spent on their equipment and maintenance, and the value of the information thus obtained amply justifies the expenditure. In our special field precedent is not so widely known, and yet it has been fully established. During the past fifteen years the United States Department of Agriculture has been conducting a biological survey of North America, upon the lines of which, as elaborated by Dr. C. Hart Merriam, we would propose to establish our work in the Old World. Through Dr. Merriam's energy and experience the technique of exploration of this kind has been perfected in the most minute detail as regards both the collecting of specimens and, what is even more important, the taking of proper observations. This work has resulted in an entirely new conception of the geographic distribution of the life of North America, to say nothing of a vast increase in our knowledge of the fauna and flora of the entire region. So successful has this work been, and so economically conducted, that biological exploration on land, on a large scale, can no longer be regarded as an experiment. By arranging the work in accordance with these tested methods, with

which both of the writers have become well acquainted, the Carnegie Institution would be spared all danger of risking support to an untried venture or in experimenting with partly understood methods.

4. *Impossibility of Carrying on the Work in Any Other Manner.*—Though previously alluded to, it may be well to state more explicitly the reason why this work, if done at all, must be carried on in the manner here proposed. It might be supposed that the material required for this investigation can be readily secured by exchange or purchase, or perhaps that it might be examined by visiting different museums. But such is not the case. The material does not exist, and, strange though it may appear, it would be no more impracticable to attempt the study of the fauna of the remotest sea bottom without the aid of specially equipped expeditions than to undertake the revision of the European members of a genus of mammals or birds without first procuring material in the same manner; and what is true of such relatively small questions is equally so of the broader problems on which the proposed work would bear. If all the data now existing in museums were brought together it would be an insignificant fragment of the mass required.

5. *Qualifications of the Writers.*—The qualifications of the writers for carrying on the proposed survey are perhaps best indicated by the appended bibliographies of papers published by them dealing with the problems under consideration.

Papers by Leonhard Stejneger Bearing on the Subjects to be Investigated.

The following list is limited to the more important papers dealing exclusively with the Palearctic biota. Papers of a more general character, such as descriptions of the countries visited and of the exploration trips undertaken in the region treated of, are not included.

1871. Ornithologische Notizen aus Meran, Sud-Tirol. Journ. f. Orn., 1871, pp. 122–124. "Nachtrag," tom. cit., pp. 462, 463.

1873. Ornithologisches aus Norwegen, I and II. Journ. f. Orn., 1873, pp. 304–307

1873. Norsk Ornitologisk Ekskursjons Fauna (Christiania), 8vo. XVI and 111 pp. and 4 lith. pl. (A pocket manual of Norwegian birds.)

1874. Norsk Mastozoologisk Ekskursjons Fauna (Christiania), 8vo. VIII and 31 pp. (A pocket manual of Norwegian mammals.)

1875. Ornithologisches aus Norwegen, III. Journ. f. Orn., 1875, p. 167.

1878. Ornithologisches aus Norwegen (IV). Ornith. Centralbl., 1878, p. 109.

1878. Underslgæten Lanius. Arch. f. Mathem. Naturvid., 1878, pp. 323-339. (A monograph of the subgenus Lanius.)

1879 Bidrag til Vestlandets ornithologiske Fauna. Nyt. Mag. Naturvid., 1879, pp. 141-148. (Contributions to the ornithology of western Norway.)

1882. Andet Bidrag til Vestlandets ornithologiske Fauna. Nyt. Mag. Naturvid., 1882, pp. 111-124. (Contributions to the ornithology of western Norway, second installment.)

1882. Outlines of a Monograph of the Cygninæ. Proc. U. S. Nat. Mus., V, 1882, pp. 174-221. (The nearctic and palæarctic swans treated of in detail.)

1883. Contributions to the History of the Commander Islands No. 1. Notes on the Natural History, including descriptions of New Cetaceans. Proc. U. S. Nat. Mus., VI, 1883, pp. 58-60.

1884. A brief Review of the Lagopodes belonging to the group Attagen. Zeitsch. f. ges. Ornith., I, pp. 86-92 and pl. V. (Monographic essay on the Ptarmigans.)

1884. Pseudototanus guttifer (Nordm) Zeitsch f. Ges. Ornith., I, pp. 223-229 and pl. X. (Rediscovery in Kamchatka of this bird, which had been lost for fifty years.)

1884. Remarks on the Species of the Genus Cepphus. Proc. U. S. Nat. Mus., VII, pp. 210-229. (A monograph of this holarctic genus.)

1884. Analecta Ornithologica. I. The Occurrence of Turdus aliciæ in the Palæarctic Region. XI. Notes on Arctic Lari. Auk, 1884, pp. 166, 360-362.

1884. Diagnosis of New Species of Birds from Kamchatka and the Commander Islands. Proc. Biol. Soc. Washington, 1884, pp. 97, 98. (Description of five new species collected by the author in Kamchatka.)

1884. Investigations Relating to the date of the Extermination of Steller's Sea-Cow. Proc. U. S. Nat. Mus., VII, pp. 181-189. (No. 2 of the Contributions to the History of the Commander Islands.)

1884. Additional Notes on the Plants of the Commander Islands. Proc. U. S. Nat. Mus., VII, pp. 529-538. (No. 4 B of the Contributions to the History of the Commander Islands.)

1884. A New Species of Woodpecker from Kamchatka. Auk, I, pp. 35, 36.

1884. Notes on the Genus Acanthis Auk, I, pp. 145-156. (A monographic paper on this holarctic genus.)

1884. Ueber einige Formen der Untergattung Anorthura. Zeitschr. Ges. Orn., I, pp. 6-14. (Monographic; holarctic.)

1885. Results of Ornithological Explorations in the Commander Islands and in Kamchatka. Bull. No. 29, U. S Nat. Mus., 382 pp. and 9 plates.

1886. The British Marsh-Tit. Proc. U. S. Nat. Mus, IX, 1886, pp. 200, 201. (Described as new.)

1886. Review of Japanese Birds. 1. The Woodpeckers. Proc. U. S. Nat. Mus., IX, pp. 99-124.

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II. Problems.

1. *Relationship Between Life of Northern America and Northern Europe and Asia.*—With regard to the relationship of the life of the northern portion of the Old World to that of northern America there exists the greatest diversity of opinion. Certain writers hold that the life of each of the two great northern land masses shows enough peculiarities of its own to warrant the recognition of a separate *Nearctic region*, and *Palearctic region* of primary rank, each, therefore, coördinate with such biogeographic areas as the *Australian region*, *Ethiopian region*, etc. Others equally competent to interpret the facts regard the biota of the two areas as practically identical, so that only when united to form a *Holarctic region* do they become strictly comparable with the other primary geographic divisions of the earth's life. In general European writers favor the former view, while Americans unanimously support the latter. That such radical difference of opinion can occur is mainly due to the fact that each party bases its assumption on confessedly inadequate data, due to the total lack of such definite information concerning the life of Europe and Asia as is now available for northern America. As an instance of the uncertainty which exists with regard to the actual relationship of the life of the *Palearctic* and *Nearctic regions*, the flowering plants, among the most carefully collected and studied of organisms, may be cited. In the latest edition of Gray's Manual about 425 species are given as common to the northeastern United States and the *Palearctic* region. Since the publication of this work many of these have been critically studied and found to be in reality aggregates of numerous closely related forms, no one of which occurs on both sides of the Atlantic. The genera *Agrimonia*, *Linnæa*, and *Antennaria* are conspicuous examples. As yet, however, no systematic general study of these species has been undertaken, so that it is impossible to say what conclusions in regard to the relationship of the life of the Old World to that of America may be drawn from the apparent similarity in this large number of species.

2. *Life Areas of Northern Europe and Asia.*—Equal divergence of opinion exists on the question of the manner of distribution of life within the two great land areas. The fact is generally admitted by American writers that the life of the so-called Nearctic region is arranged in zones, or belts of relative homogeneity, bounded by certain isothermal lines, and, like the isotherms, extending in a general

east and west direction but subject to deflection northward by low, hot plains, and southward by mountain ranges; also that in the ascent of a mountain rising from the area occupied by one of the more southerly life zones, assemblages of animals and plants closely corresponding with those of the more northerly zones are successively encountered in regions whose altitude produces the requisite lower temperature. By nearly all students of the *Palearctic* region, on the other hand, it is supposed that the life areas of Europe and Asia form large blocks, of no definite form and bearing no close relation to isothermal boundaries. Thus, there is commonly recognized a *European* subregion extending from the barren shores of the Arctic Ocean to the luxuriant coast of the Mediterranean, and embracing the great mountain mass of the Alps. This subregion is by some writers contrasted with a *Siberian* subregion, considered as of equal rank; by others united with it to form a *Europasian* subregion. Other divisions of the *Palearctic* region are the *Manchurian* subregion, the *Mediterranean* sub-region, the *Eremian* sub-region, etc. In all this there is the greatest diversity of opinion, no two writers agreeing as to the number, names, or boundaries of the subdivisions. In only one feature is there uniformity, in the absence of that universal conception of zonal arrangement of the life areas that characterizes the work of American writers on the zoögeography of the *Nearctic* region. In fact, one European author has recently gone so far as to point out reasons why zonal distribution can not exist in certain portions of the Old World.

But is it probable that such a radical difference in the distribution of the life of America and Asia actually exists? Does one law of distribution hold good in one continent and a wholly different law in the other, even though many of the animals and plants of the two regions are by all writers admitted to be closely related? Or are the life zones of America represented by strictly homologous areas in the Old World, dependent on the same underlying physiological laws? These questions are perhaps the most important now before the student of the general dispersion of life in the northern hemisphere.

Their answer was hinted at by several writers of half a century ago, whose work is now generally overlooked. Agassiz, in 1850, went so far as not only to recognize the zonal distribution of life in the Alps and the region lying to the north of them, but also to suggest the correlation of the life zones which he had observed in Europe with those of eastern North America. One of the present writers

has spent much time in the attempt to follow out this course of investigation, but beyond the discovery of much scattered evidence no result has been possible, owing to the absence of material. He has been able, however, to construct a preliminary map of the life zones of Europe, and his success, with the very limited data at his disposal, points toward a speedy and logical settlement of the question as a certain result of properly conducted explorations.

3. *Former Glaciation and Its Bearing on the Present Distribution of Life.*—It is scarcely necessary to allude to the close connection between the more directly zoölogical questions and those of Post-Tertiary paleontology and geology, but we may call attention to the fact that recently attempts have been made, on biological grounds, to belittle the extent and intensity of the glacial phenomena in Europe during the period known as the Ice Age. It has been argued, with considerable plausibility, that the conditions in Europe at that time were such that a universal glaciation must have been an impossibility, and that, so far from being colder, the climate of Europe was then milder than at present. In our opinion, such theories have no actual foundations, and we believe that it is only defective knowledge of the minute details of the fauna and flora of the entire northern world that is responsible for the fact that they can be propounded and defended, a view publicly expressed not long ago by one of the present writers. Nevertheless, it can not be denied that with the present status of our knowledge the discussion might go on indefinitely. A better knowledge of the life zones of the Old World would probably also greatly promote the correct understanding of the biological side, at least, of the inter-glacial question.

If the climate of Europe in the glacial period is the source of much contention between widely diverging views, that of Siberia and of the Asiatic Pacific coast is still more in doubt. While nobody may be found to advocate a glaciation equaling in extent, intensity, and duration that of Europe or North America, there is no lack of authorities who maintain that the climate must have been quite severe and that glaciers were not absent. On the other hand, an author has within the last two years defended the proposition that the climate of that region did not differ materially from that of today.

4. *Manner of Expansion of Faunas.*—It has been supposed that a large proportion of the present fauna of Europe on the one hand

and of America on the other had its origin somewhere in central Asia, having spread fan-like in both an easterly and westerly direction. From various paleontologic as well as biologic facts of a more or less fragmentary nature, it is generally held that such invasions took place during glacial times, both before and after the maximum glaciation was reached. But there are clear indications that such expansion from Asia to Europe and to America is going on today. In a paper read before the Biological Society of Washington, one of the present writers was able to demonstrate this fact with regard to a limited number of species of mammals and birds in northern Russia which in their westward march are nearing the Atlantic or have reached it. The data for northeastern Siberia and Alaska are still more fragmentary, yet there are indications pointing to a similar eastward movement of the Siberian animals. It will be easily seen how important it is to determine as far as possible the extent and volume of this invasion.

While it would seem from a contemplation of the cases known that this expansion is made gradually by the species extending its range at the periphery of its distribution, it can not be denied that some animals may have spread by means of sudden irruption of great numbers into hitherto unoccupied territory. As examples of such explosive movements we may cite the occasional invasion of Europe by great numbers of the Asiatic sand grouse, as in 1863 and 1888; similar, though more frequent irruptions of the spotted nutcracker; and the well known wanderings of various rodents, such as the lemmings. A full study of these phenomena may have a very important bearing upon the question of the proper interpretation of many finds of fossils. If a future paleontologist were to find in England the bones of the sand grouse in some deposit from 1863 he might conclude that that species had been native of Great Britain, and that consequently a climate like that of the central Asiatic steppes prevailed in England in 1863. As the lemming during its periodical wanderings may deposit its bones far beyond the limits of its proper life zone, it is plain that the discovery of its fossil or subfossil remains is not in itself sufficient proof of the former existence of that life zone at the locality in question.

5. *Relation of the Cave Fauna of Europe to Existing Life.*—It is well known that caverns in various parts of central and southern Europe contain the bones of animals of a kind very different from those now inhabiting the region. In many instances these repre-

sent extinct species; but it frequently happens that the remains are referred to members of the fauna now existing in other regions. In the majority of such cases the animals are now found far to the northward, the unavoidable conclusion being that when the cave deposits were formed the climate was such as to permit their existence in the neighborhood, yet in few instances has sufficient critical comparison been made between the cave remains and the bones of the existing species they are supposed to represent. Hence it is impossible to determine whether the time which has elapsed since the climate of central Europe was such as to allow the existence of arctic animals is sufficiently great to have permitted the survivors to become in any way differentiated, or whether it is so short that no changes have taken place. In one instance—the lemming of the cave in Portugal—in which a comparison of this kind was made, the result showed that the remains differ materially from those of the animal now living in Norway. To continue such comparisons would be an important branch of the investigations here proposed.

6. *Migration of Birds in Northern Europe and Asia.*—Palmén has clearly demonstrated that the routes of certain migrating birds furnish good indications of former land connections, or former shore lines, as the case may be, as well as of the road by which the species have extended their range from the original center of distribution. For the solution of the problems before us it is therefore of the utmost importance that these migration routes be determined and analyzed in detail for as many species as possible. Palmén himself was able to trace several such routes in Europe and western Asia which have shed considerable light on former distribution of land and water. I need only refer to the route from Novaya Zemlya to the White Sea, to the Gulf of Finland along the Baltic, across Holstein, to the North Sea and southern England and Ireland, thus plainly outlining the shore of the Arctic marine transgression at a certain period in glacial times. Similar routes across the Mediterranean have been shown to follow the old land bridges between Europe and Africa. In Europe these routes have been indicated for a number of species, though many birds are simply referred to them on general principles. In Asia, however, the work of determining these routes is still in its hypothetic stage. One of the present writers as far back as 1885 broadly outlined the migration route of at least two species from Bering Strait to India, showing it to lie in an entirely

unsuspected direction through the interior of the continent, and he also indicated the routes of other species in a tentative way.

Pressure of other work, as well as lack of material, has prevented further research in that direction, and no one else has taken up the subject or is liable to do so. A year ago, however, he was led to investigate the routes of an Old World migrating bird which extends its range into Greenland and eastern North America on the one side and into Alaska on the other. He was able to prove that the bird in these two localities is represented by two subspecies, one of which migrates in winter to western Africa, the other to India. The routes thus traced clearly indicated the way by which the species originally invaded America. To better illustrate the importance of the investigation we may be permitted to quote the concluding paragraph of the generalization based upon it, as follows:

"It seems that one more lesson can fairly be drawn from the differentiation of the Greenland race, viz., that the Greenland-Iceland-England route must be considerably older than the Alaska-Chukchi-Udski route, since it has resulted in the establishment of a separable race. A consideration of the further fact that no regular migration route could have been effected between Greenland, Iceland, and Great Britain during the present distribution of land and water in that part of the world also leads us back to a period when the stretches of ocean now separating those islands were more or less bridged over by land. For such a condition of affairs we shall have to look toward the beginning of the Glacial period. At that time it must, therefore, be assumed that the Wheatear extended its range into Greenland. The advent of the typical form into Alaska, on the other hand, is probably one of very recent time, an assumption corroborated by the somewhat uncertain and erratic distribution of the species in that northwestern corner of our continent." (Proc. U. S. Nat. Mus., XXIII, 1901, p. 477.)

The exceptionally satisfactory results of this investigation were due to the coincidence of a fairly good series of specimens in the National Museum with the fact that the difference between the two races was one of length of wing, and that consequently the measurements by European authors could be utilized. If the distinguishing characters had been of a different nature, the material at hand would have been insufficient.

It will thus be easily seen that if we had the material we could determine these routes for many species, and thus secure valuable facts for a correct interpretation of many of the most important problems now under discussion.

7. *Origin of the Present Fauna of Europe.*—Considerable light has of late years been thrown on the question of the origin of the European fauna; yet while certain general propositions may be said to have gained common acceptance because based on fairly convincing evidence, many others are based chiefly upon more or less plausible theories, for which the proof in the shape of sufficient material is still lacking. On the other hand, there are numerous important questions where the opinions and theories of those best equipped to judge are diametrically opposite, not because the nature of the questions is such as to preclude an indisputable settlement, but simply because there is not material in any one museum, nor even in all museums, adequate to establish the status of the species or their geographical distribution.

It would be impracticable in the present connection to go into detail with regard to all these questions or to prove the above assertion for the various classes of animals. It must suffice to mention a few flagrant cases which may stand as examples of the rest.

One of the most important problems, as far as the origin of the European fauna is concerned, is the question whence came the animals which Doctor Scharff has termed the "Arctic migration." He, with many others, contends that until toward the end of the glacial period there existed a continuous land connection between America and Europe, far north between Greenland, Spitzbergen, and Scandinavia, the latter being again connected by a land bridge with Scotland across the North Sea, and England with France. Across this continuous land bridge these animals are supposed by him to have wandered into central Europe. One of the writers has tried to disprove this hypothesis, both on physiographical and biological grounds, and has advanced the theory that these animals reached central Europe and Great Britain from western Siberia before the maximum glaciation, and that from Scotland they extended into western Norway. Unfortunately, however, there is nowhere any material gathered together which would help settle the question.

It is unnecessary to here detail the gaps in our knowledge which make a discussion of this question a mere guessing contest. Suffice it to say that some of the most elementary facts concerning the relations and distribution of some of the largest, most conspicuous, and most important species are in dispute. Indispensable details as to the distribution and relationships of the ermine, the lemmings, the deer, the reindeer, the grouse, the ptarmigan, etc., are still lacking and, except by some lucky chance, may not be forthcoming for a long

time. Vague notions abound but there is little certainty. And if our knowledge of the big and conspicuous animals is so defective, what may be expected of the obscure and rare forms?

We have selected the above question as an illustration because it has so direct a bearing upon the history of our own North American fauna; for clearly, if such a continuous land bridge existed between the Old and the New Worlds toward the early days of the glacial period that reindeer and ermine and lemmings could pass from America to Europe, we are justified in inquiring into the question which animals wandered from Europe into America by the same bridge.

But while we do not deem it necessary to go into further detail, it may be mentioned, as highly instructive in this connection, that notwithstanding all that has been written as to the relationship of the animals in the Scandinavian mountains and the Alps, and in spite of all the theories based upon this fact, such as that of the gradual following up of the receding northern and southern glaciers by the arctic inhabitants of the central European plains, no satisfactory comparison in the modern sense of the animals most important in this respect has ever been undertaken. It is a fact worth noting that it is little more than a year since one of the present writers distinguished between the Scandinavian and central European varying hares. This separation made here in America, from material in American museums, emphasizes the need of the work here proposed.

8. *Special problems in Eastern Asia.*

(*a*) *Former land connection between Asia and America.*—Turning now to the eastern or Pacific side of the Eurasian continent, we find equally important problems in even less satisfactory condition. As far as they have a bearing on North American problems, it may be said that their importance to us is even greater than those we have mentioned above. Whatever we may think of a land connection in glacial times between America and the Old World by way of Spitzbergen or Iceland, there is less doubt as to the connection by way of Bering strait, and while it is doubtful whether America has received any additions to its Pleistocene or Postpleistocene fauna or flora by way of the former, there is scarcely any doubt that a large influx came by way of the latter. While there is a general feeling among biologists that such is the case, it is very difficult to prove it in detail, as was clearly shown during a recent "symposium" or discussion of the matter in the Washington Biological

Society, during which there were adduced many general observations tending to substantiate such a claim, but very little positive evidence for or against. This is equally true of zoology, botany, paleontology, and geology. The tendency was to regard Bering strait as the probable route; yet it must not be overlooked that so prominent an authority as Dr. Henry Woodward, as lately as 1894, could state with regard to the points brought out by Dr. George M. Dawson, in a paper then under discussion, that he regarded Dawson's paper as proving that the Aleutian islands are the old high road for the mammoth and other mammals from Asia into North America in Pleistocene times (Quart. Jour. Geol. Soc., Vol. L, Feb., 1894, p. 9). In looking for the evidence of this, one is struck by the extremely meager details upon which such sweeping generalizations are based, as well as by the uncertainty of even these few facts. Thus the mainstay of Dr. Woodward's contention is a record of a few bones of mammoth having been found by "some men, probably Russian promyshleniks," on Unalaska Island in 1801.

(*b*) *Japan.*—The fauna and flora of the outlying islands and peninsulas of northeastern Asia is fully as much a key to the situation there as that of Great Britain, Scandinavia, and Spain is to similar problems in Europe. If we had the material to properly interpret the present and past distribution of life in the Chukchi peninsula, Kamchatka, the Kurils, Sakhalin, and the Japanese islands we might easily trace the history of the migrations of the land animals in that part of the world and the expansion of the Asiatic fauna and flora into the northern portion of our own continent. But no such material is at hand. The U. S. National Museum probably has the best collection of Japanese birds, and one of the present writers has devoted years to its study, and published numerous papers upon details of Japanese ornithology; but lack of means has prevented the completion of the work, for which a large amount of manuscript has been accumulated, but never published, simply because he has been unable to secure the necessary material. He has also in hand an extensive work on the herpetology of those regions, which is well under way toward completion; but it can only be regarded as a preliminary reconnaissance, the material being too fragmentary for an exhaustive treatise, though based upon the two largest collections of the kind, those of the U. S. National Museum and of the Imperial University in Tokyo. Our knowledge of the Japanese mammals is even less advanced than it was seventy years ago, and there is absolutely no material in any museum. Large por-

tions of Japan are complete *terrae incognitae* as far as the higher animals are concerned; thus, for instance, the whole large island of Shikoku. The Japanese themselves have had so many other problems to solve that their scientific activity has as yet yielded but slight results in our direction.

(*c*) *Kamchatka.*—As for the peninsula of Kamchatka very little biological work has been done there. One of the present writers has had the good fortune to be able to collect at one place in the southern part and has published on the birds of the region. He was enabled to draw some general conclusions as to the character of the fauna, and to indicate, although in a very tentative way, the insular nature of its biota, conclusions since corroborated by an eminent geologist. But beyond these hesitating suggestions nothing definite can be asserted from lack of extensive and positive information.

(*d*) *Kuril Islands.*—The Kuril Islands, which now form the connecting link between Kamchatka and Japan, have not yet been reached by the modern biologist, except that a Japanese botanist has given an account of the flora to a great extent based on antiquated information, while one of the present writers has published a preliminary account of the birds, also based to a great extent upon old and uncontrollable records, his own visit having been too brief for any extensive collecting.

Sakhalin has been studied to some extent by Russian scientists, but the results are partly inaccessible because in the Russian language, and because the material collected is now scattered.

(*e*) *Mainland of Eastern Asia.*—The adjacent portions of the Asiatic mainland, the tracts adjoining the Okhotsk Sea, and the Amur region are among the most important localities for the elucidation of the questions we propose to solve. Fifty years ago they were the scene of the explorations of Middendorf, Schrenck, and Radde, and at that time our knowledge of those regions was ahead of that of our own great West. How different today! Biological science has taken a new start since the days of those pioneers, and excellent as their work was for that time, it is utterly antiquated and inadequate today. Nothing worth speaking of has been added, and for present purposes the work is nearly useless because of its lack of detail and the absence of sufficient material in large series to substantiate the results.

These large series of specimens from the entire circumpolar area are among the greatest desiderata of biologists dealing with these problems. There is no museum in America which has even a repre-

sentative collection consisting of single specimens of typical forms occurring across the vast continent from England to Kamchatka, and no museum in the world—nay, not even all the museums put together—could furnish such series of any large group of animals as are required by modern methods.

(*f*) *Miscellaneous allied questions.*—It would carry us too far were we to go into detail, pointing out all the important problems which wait for a solution through a study of the life of Siberia and an intelligent comparison with the corresponding area of our own continent. The evolution of the steppe and tundra animals and their spreading at periods when the steppe or the tundra life zones temporarily at least extended into other regions; the significance of the marine life, the seals, crustaceans, sponges, etc., in Lake Baikal; the question as to the efficiency of the marine transgression of the Arctic Ocean east of the Urals in establishing an effective barrier against Siberian fauna and flora expanding into Europe during glacial times, either alone or in conjunction with glaciation; the climate of the interior of Siberia during the same period and the curious mixed fauna, as shown by the fossils from the New Siberian islands and the mouth of the Yana river; the very origin of the fauna which apparently radiated from some point in that vast territory reaching as far west as the British islands and as far east as Greenland—these and many other problems are closely connected and are all waiting for further elucidation based upon the bringing together under one comprehensive system of detailed material for minute study and exact correlation.

9. *Some probable systematic results.*—One of the aspects of the proposed exploration which must not be overlooked is the great increase in the systematic knowledge of the fauna and flora of northern Asia and Europe that must result from definitely planned and executed field work. While it is impossible to form any accurate estimate of the probable results in this direction, a general idea of the outcome may easily be obtained.

Before the beginning of the work of the Biological Survey of the Department of Agriculture, the known mammal fauna of North America consisted of about 350 species. As a result of the work carried on by this Survey, the number of recognized forms is now between 1,600 and 1,700. In that portion of the Old World which we propose to explore there are now known about 600 mammals. An increasae at least as proportionately great as in America is to be expected; in other words, we may look for the discovery of nearly 2,000 mammals

now quite unknown to science. It may be mentioned that as the result of some very fragmentary explorations (part of which were directed by one of the writers) in the best known part of Europe more than 60 new mammals have been discovered during the past few years.

In other classes a very great increase of the same kind is to be expected, though probably to a less extent than in mammals. Add to this the fact that the geographic distribution of practically no widely spread species of Old World vertebrate is sufficiently well known to permit its range to be mapped, and the importance of this branch of the inquiry will be appreciated. As already pointed out, much remains to be done in the study of those plants whose range is supposed to cover parts of both hemispheres. From the results already obtained it seems highly probable that a new conception of the relationship of the members of most *Holarctic* genera would result from such work systematically conducted.

III. METHOD OF CONDUCTING THE WORK.

1. *General organization.*—The general organization proposed is as follows: The work would be conducted under the joint supervision of the two directors, each of whom would take special charge of the problems toward which his previous studies have been chiefly turned, and each paying particular attention to the region in which work has already been begun—that is, one at the extreme east, the other at the west of the vast region which it is proposed to explore. The care of the financial and administrative details of the survey would be placed in the hands of an executive officer selected with special regard to business ability. The force of field naturalists would, when fully organized, consist of from twenty to thirty observers and their assistants, specially trained for the work. In Washington a building should be provided for laboratories and temporary installment of the collections.

2. *Organization of field force.*—While the organization of the field force must to a large extent depend on circumstances which can not be foreseen, it is possible to gain a clear idea of the general course to be pursued. Men already trained in the methods would be at once placed in the more readily accessible regions, accompanied by assistants who, when sufficiently experienced, would in turn become the educators of the new men, and push on into more remote regions. In this manner an efficient force would be built up and the work carried on uniformly throughout, a consideration of the utmost importance. Among the field naturalists those who show aptitude for

such work would be expected to elaborate their results for publication on their return from the field and while yet in the employ of the survey.

3. *Organization of office force.*—At the beginning of the work the office force would be small; probably a typewriter, a laborer, and a taxidermist would be sufficient in addition to the directors. Preparation of outfits would be attended to by the field agents, aided by the taxidermist. The latter would thus be quickly trained to carry on this part of the work, and to superintend it during the remainder of the survey. Material might be expected to arrive within three or four months, and this would necessitate the services of another man to attend to cataloguing and labeling. When the full number of agents are in the field. an addition to the cataloguing force will be required; exactly how much can not be estimated, but probably two or even three men. The reception of specimens will also call for an addition to the taxidermist's force, as well as for the employment of one and later more preparators for cleaning skulls, mounting plants, etc. As soon as large mammals begin to accumulate, the services of a tanner will be required for putting the specimens in a condition of safety.

The force thus outlined will doubtless be sufficient for running the work during the first five or six years. Later, as publication of results increases in importance, and extensive work is being done on the collections, an increase will be needed. Just what this will amount to can not be foreseen, but the lessening of the amount of field work will doubtless compensate for it, so far as total expense is concerned.

4. *Relationship to other institutions.*—The activities of the force of field naturalists which it is hoped would be organized should result in the bringing together yearly of from 10,000 to 20,000 specimens of vertebrates alone, in addition to which there will be large quantities of invertebrates, plants, etc. Provided it be not the policy of the Carnegie Institution to establish a museum of its own, the disposition of this material will demand careful consideration. The plan to which we would direct the attention of the Executive Committee is that temporary quarters for the collections be established in Washington, where they may be kept for elaboration and study during the course of the survey. Eventually the collections and sets of duplicates may be distributed, at the discretion of the directors of the survey and under the approval of the Carnegie Institution, to other institutions and to specialists who may have aided in the elaboration of material.

IV. Estimates of Cost.

The following estimates can be regarded as nothing more than a tentative outline of the probable expenses. While the Biological Survey of the U. S. Department of Agriculture furnishes a reliable standard, so far as methods are concerned, its expenditures have no direct bearing on those to be expected in the present case. This is owing to the much larger area to be explored, and also to the great distance between headquarters and the field to be worked.

The estimates here presented* are believed to represent the maximum outlay that may reasonably be expected. During the first years of the work a considerable saving is to be looked for, which will allow for the accumulation of a balance to be applied to unforeseen expenses during the later phases of the enterprise.

Estimate on Basis of First and Subsequent Years.

I. Field work:	
First year	$22,800
Subsequent years, per year	37,050
II. Administration and care of collections:	
First year	22,200
Subsequent years, per year	22,650
III. Contingent:	
First year	5,000
Subsequent years, per year	5,300
Total:	
First year	50,000
Subsequent years, per year	65,000

Estimates on Basis of One, Two, and Three Years.

I. Field work:	
First year	$16,150
Second year	22,300
Third and subsequent years	36,550
II. Administration and care of collections:	
First year	16,570
Second year	18,170
Third and subsequent years	21,640
III. Contingent:	
First year	2,280
Second year	4,530
Third and subsequent years	6,810
Total:	
First year	35,000
Second year	45,000
Third and subsequent years	65,000

*Details omitted in printing.

V. Opinions on the Plan.

We here subjoin the opinions of a few well known naturalists on the work proposed in the foregoing plan.

[*Dr. J. A. Allen, American Museum Natural History, N. Y., to Dr. Stejneger.*]

New York City, *October* 19, 1902.

My Dear Dr. Stejneger:

I have read with great care and interest your letter of October 13, outlining the plan and purposes of a biological survey of the Palearctic region, as proposed by you and Mr. Gerrit S. Miller, Jr., for the consideration of the Trustees of the Carnegie Institution. I am deeply impressed with its magnitude and importance. It seems like a project so far reaching and beneficent in its influence upon the progress of science that its accomplishment is almost beyond hope, even in these days of great undertakings. Such an enterprise is of course beyond the means of any individual, or of any of our museums or universities. Unless it can be taken up and carried forward as a part of the work of the Carnegie Institution, we shall have to depend in the future, as in the past, upon the independent and unco-ordinated efforts of individual explorers and collectors for any advance in our knowledge of the biota of this great region and its relation to that of North America. Collections formed in this way are in the first place unsystematic, usually limited to a few classes of objects and to a few restricted areas, and are worked up separately by specialists limited to small amounts of material and without opportunities for its proper comparison with allied material collected previously at other times and places. Under such unfavorable conditions some progress has been made, but under such methods generations will pass before any very thorough and comprehensive knowledge is gained in regard to the constitution and relationships of the biota of the different faunal and floral areas of the Holarctic region.

What is needed is a thoroughly organized and comprehensive scheme of exploration, carefully planned and systematically organized as respects field parties and the areas to be covered by them; a main base for the outfitting for field work, and to which the collections should be returned, and thence distributed to experts for elaboration. It is evident that no government will or can undertake such a survey for obvious geographical and political reasons, and

that no museum or other institution of learning can assume the financial responsibility. The field is thus clear for the work you have outlined, and unless financial aid can be supplied by the Carnegie Institution for this well conceived undertaking it will doubtless remain merely the dream of its projectors.

The biota of the Palearctic region is at present only very imperfectly known, and very little material, comparatively speaking, has been collected, and that only in a casual and superficial way. Moreover, this is widely scattered and practically inaccessible to the students of any special group or area. Hence its bearing on general problems, as to former land connections between Eurasia and North America, the place of origin of groups common to the two continents, the relation of particular phases or aspects of plant and animal life to their special environment, can not be determined. Under your plan of consecutive and systematic field work for a period of years, under the most careful instructions as to methods of preparation, biologists would soon be in possession of means for the determination of all these questions, resulting in a most marked advance in knowledge along these lines, which could not fail to have also a direct and powerful influence upon the general progress of biology. The amount of material thus gathered in accordance with modern American methods of field work could not fail to be very great, and after elaboration would be available for distribution among a large number of scientific institutions, where it would have permanent value as a part of the records of science and as a basis of future comparative research. Its elaboration would give opportunity for some of the younger investigators, to whom some of the material would naturally be submitted, to show their ability for research, and thus promote one of the primary purposes of the Carnegie gift, the discovery of talent and favorable opportunity for its development.

Our United States Biological Survey, as conducted by Dr. Merriam, is an illustration of what may be accomplished by the use of moderate means expended under wise direction. You and Mr. Miller have been more or less associated with this work, and are familiar with the most approved modern methods of field work. I am sure also that you are conversant with all the details of an effective inception of the great undertaking you have so intelligently proposed, and sufficiently in touch with all the leaders in biological investigation to be able to give wise suggestions as to the apportionment of the material for elaboration among those best fitted for such work.

Your proposal for a biological survey of the great Palearctic region has thus my most earnest endorsement, and my best wishes for its favorable consideration by the administrators of the Carnegie fund, a portion of the income from which, it seems to me, may most fittingly be devoted to such work.

Sincerely yours, J. A. ALLEN.

[*Dr. Theo. Gill, Smithsonian Institution, to Dr. Stejneger.*]

WASHINGTON, D. C., *October* 14, 1902.

DEAR DR. STEJNEGER:

I have carefully read the copy of your and Mr. Miller's detailed proposition to the Carnegie Institution for the biological exploration of the Palearctic region, and the more I consider it the more favorably it impresses me. I presume it will be a revelation to most of our friends that the zoogeography of Europe is in need of further elucidation, and yet all your statements are perfectly true. Not only is the geographic distribution of life in the Palearctic region very imperfectly understood, but also in every department of zoology with which I am familiar uncertainty prevails respecting the species and the value of differential characters. Such questions, all of primary importance in biology, can only be settled by systematic exploration of the kind you have planned. You make especial mention of the mammal, bird, and reptile fauna, and I can bear witness to the fact that the fishes and mollusks are enveloped in a similar cloud of doubt. No two observers have independently reached the same conclusion as to the systematic relation of any large and varied group.

I hope you will be successful in obtaining the aid of the Carnegie Institution in undertaking the proposed exploration. It will interfere with no work now being done, and no other institution can possibly undertake it. Such an "American invasion" will be of benefit to all parties.

Very truly yours, THEO. GILL.

[*Dr. David Starr Jordan, President Stanford University, to Dr. Stejneger.*

STANFORD UNIVERSITY, CAL., *October* 17, 1902.

DR. LEONHARD STEJNEGER,

Smithsonian Institution, Washington, D. C.

DEAR SIR: I am deeply interested in your project for a natural history survey of northern Asia. As you know, my own work in the

Pacific is largely connected with such a survey, and until the fauna of eastern Asia is well known all our work on the fauna of the western Pacific must be tentative. The fauna and flora of western North America and of the Pacific Ocean are largely derived from Asiatic sources, and until we have studied Asia we are in the position in which students of American history would be if the history and peoples of Europe were practically unknown. The parts of biology which deal with evolution, with geographical distribution, and with relations to environment would be immeasurably benefited by such a survey as you contemplate, and I sincerely hope that the Carnegie Institution may see fit to provide for it.

Under present auspices, it will be wholly impossible to do this work within a century. The small funds the American universities can spare go but a little way, and so far as the European institutions are concerned, all that has been done is of the slightest importance in proportion to the magnitude of the problems.

There could, therefore, be no interference with the work of others. In fact, all interested in these problems would derive their best material from this survey, and their energies would thus be enlisted in it.

The plan as detailed to me seems to me entirely practicable at a moderate expenditure each year for a number of years.

I need not say that there can be no possible question as to the fitness of yourself and Mr. Miller to direct this work. American naturalists have long recognized in you one of the master minds of zoology, and you have been as successful in practical work, as that of the Fur Seal Commission, as in the research work of pure science.

Not one of the various projects submitted to the Trustees of the Carnegie Institution interests me personally so much as yours, and none can have a more far reaching relation to the development of biological science.

It is our opportunity to claim the Pacific Ocean and to study its shores and its life with the thoroughness that our colleagues in Europe have devoted for a century to the Atlantic.

Very truly yours, DAVID STARR JORDAN.

President Stanford University.

PLAN FOR A BIOLOGICAL SURVEY OF SOUTH AND CENTRAL AMERICA

By C. Hart Merriam

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Washington, D. C., *June* 13, 1902.

To the Board of Trustees of the Carnegie Institution.

Gentlemen: I have the honor to submit herewith a plan, and to ask a grant for carrying on, a Biological Survey of Central and South America, with special reference to the terrestrial vertebrate fauna and the woody plants (particularly trees and shrubs), these types having proved the most useful in the study of geographic distribution.

Respectfully submitted. C. Hart Merriam.

The Field.—While sporadic collections have been made in many parts of Central and South America, no consecutive systematic studies of the faunal areas have been attempted. It is probable that a biological survey of the kind here outlined would result not only in the discovery of a large number of genera and several thousand species new to science, but also in the accurate outlining of the life areas of the region, and in the acquisition of material which would admit of comprehensive studies as to the origin and relationships of the South American faunas.

Plan.—The plan contemplates the collection of material and the elaboration of results. This means the employment of a corps of competent trained field naturalists and the conduct for a number of years of field expeditions in unknown and little known regions. It is expected that the principal assistants would give part of their time to the personal direction of work in the field and part to the elaboration of results. The specimens collected, together with full field notes relating to the same, would be sent to the central office for study.

Since most of the type specimens of the known Central and South American species are in foreign museums, it would be necessary for those in charge of the work to visit those museums in order to ascertain just what these species are. An effort would be made to duplicate these types in order that we may have in this country the necessary units for comparison.

Methods.—The methods recommended are those of the Biological Survey of North America, of which your petitioner was the organizer and of which he has had continuous charge for the past 14 years. These methods, briefly stated, consist in the study of faunas and the collection of specimens in the field, followed by the elaboration of results. The field expeditions, which have now covered most of North America from southern Mexico to Hudson Bay, Great Slave Lake, and various parts of Alaska, are in charge of trained field naturalists who note the changes in passing from one faunal belt or area to another, establish points along their boundaries, and make collections of the characteristic species. The parties are small and compact, usually consisting of three men—the assistant in charge, a competent field collector, and a man who acts as cook and packer (or boatman). They travel by wagon, pack outfit, or boat, according to the requirements of the country. Experience has shown that a given number of men accomplish vastly more work when divided into small parties than when united in large parties.

Date of beginning.—As the preliminary arrangements would take some time, it is recommended that a definite answer be given as early as practicable, and, if favorable, that the initial appropriation be made available in September, 1903.

Cost.—As it is impossible to obtain at the outset a sufficient number of competent trained field naturalists, it is recommended that the work be begun with a rather small grant, and that thereafter the annual appropriation be increased in accordance with the following estimates:

Estimate for fall of 1903	$3,000

ESTIMATE FOR FIRST YEAR.

Salaries of two chief assistants, at $3,000	$6,000
Salaries of two field collectors, at $1,500	3,000
Salaries of two field collectors, at $1,200	2,400
Field expenses, including transportation, subsistence, collecting outfit, and materials	5,000
Tansportation charges on specimens to the United States	500
Salary of stenographer and clerk	1,000
Cases for specimens	500
Total	$18,400

ESTIMATE FOR SECOND YEAR.

Same as for first year	$18,400
with the following additions:	
One chief botanist (or dendrologist)	3,000
One assistant botanist	1,500
Traveling and field expenses of botanists	2,000
1 field naturalist. at $2,000	2,000
1 preparator at central office	1,200
Expenses at central office and other miscellaneous expenses	300
Visits to foreign museums to examine type specimens from South and Central America	500
1 administrative assistant at central office	2,000
Total for second year	$30,900

It is recommended that after the second year an annual increase of ten per cent on this amount be provided for at least ten years.

No estimate for illustrations and reports is here made, as it is supposed that these will come under the general provision for publications.

General remarks.—With the possible exception of Africa, less is known of the fauna of South America than of any other part of the world. In order to secure a broad and properly balanced view of the life of the globe, it is necessary to know approximately the number of generic and specific types of each region. Until this is known, the relative values of the various faunal provinces can not be properly determined.

Recent investigations of fossil faunas in South America have developed numerous and weighty facts, previously unsuspected, as to the extraordinary composition and unlooked-for relationship of the animals inhabiting this region in the past. But so little is known of the existing fauna that the application and significance of the fossil record with reference to existing life is meager and unsatisfactory.

By far the greater part of the zoological material heretofore collected in Central and South America has found its way to foreign museums, where it is widely scattered. Publications relating to it are mainly in foreign languages, and the descriptions of species are, as a rule, so unsatisfactory that reference to the specimens themselves is necessary for positive identification. This, in the case of American workers, necessitates a trip to foreign museums. There would seem to be every reason why America should take the lead in this work; and it should be done in a broad and thorough way, so that the results would be final and of permanent value.

ICHTHYOLOGY OF THE PACIFIC OCEAN

BY DAVID STARR JORDAN

Dr. Jordan has submitted a plan for completing the study of the Ichthyology of the Pacific Ocean that includes a series of expeditions extending over a period of three years. Dr. Jordan's letter is as follows:

STANFORD UNIVERSITY, CAL., *April* 9, 1902.

The writer, with his associates, Prof. C. H. Gilbert, Prof. O. P. Jenkins, and Messrs. Snyder and Starks, instructors in Stanford University, has long had in contemplation an Ichthyology of the Pacific Ocean. To this end, they have thoroughly explored the Pacific coast from Bering Sea to the Galapagos, having already described all the known species in this region, figuring many of them. With the aid of the *Albatross*, many or most of the fishes of Bering Sea have been made known. In company with Mr. Snyder, the writer spent the summer of 1900 in Japan, securing about 800 species, 250 of them new. Last summer, with Dr. Evermann and Dr. Jenkins, was spent in the collection of the shore fishes of Honolulu, in the interest of the United States Fish Commission, obtaining about 350 species. At present the *Albatross*, in charge of Professor Gilbert, is at work collecting the deep sea fishes and invertebrates of Hawaii. This summer the writer proposes to spend in similar work in Samoa. Besides these personal excursions and the work of Garman, Bean, and others, the writer has correspondents in Japan, China, Australia, the Philippines, and elsewhere, from whom important collections are frequently received.

To complete this work properly, competent collectors should visit Peru, Chile, Patagonia, Australia, China, the Okhotsk Sea, and several of the islands of the East Indies and Polynesia. For this purpose suitable men can be found who will do the work for their actual expenses. Some of these are instructors at Stanford University; others are advanced students. So far as the American shores and islands are concerned, we may count on the co-operation of the United States Fish Commission; and the funds of the Hopkins Laboratory of Stanford University have maintained expeditions to Japan, the Galapagos, Mazatlan, and Panama. The University of Tokyo will co-operate in Japan.

But these funds are not sufficient to finish the work—at least not under many years.

To complete the survey, twelve to fifteen expeditions would be necessary, each costing from $1,000 to $1,200, if two men are sent together, and in remote regions this should always be done. With the fishes large collections of invertebrates and of other vertebrates could also be secured without much extra cost.

To take care of the collections one curator should be paid, and an adequate supply of bottles and alcohol should be at hand. All this, with the expeditions, could be provided for with a sum of $7,000 yearly for three years, besides the pay of artists and typewriters. With a smaller sum the same results could be reached in longer time.

The work of study of the collections could be accomplished without expense by professors and advanced students. Artists and typewriters, however, must receive salaries, if employed. All new species should be figured, and it would be most desirable if in a final report *all* could be figured. The figures already made by the United States National Museum could, of course, be used for this purpose.

The present writer and his associates will devote their available time to this work, in any event, but unless especially accelerated it must outlast any one man's lifetime.

I commend this to the attention of the Trustees of the Carnegie fund as a means of aiding our knowledge of the science of Ichthyology, our knowledge of the geographical distribution of organisms, and, through the collection at the same time of marine invertebrates, of extending the range of that branch of zoology.

In case a grant should be made to this end, Stanford University will house the collections until studied, after which it will send the first series to the United States National Museum and distribute the rest to the museums of the world. The books necessary in this work are already in the possession of Stanford University. The means for publication of the work as a whole are not provided for.

In case this work seems a suitable one for aid by the Carnegie fund, I commend it to the attention of your honorable Committee. I am,

Very truly yours, DAVID S. JORDAN,
President Stanford University.

EXPLORATION AND STUDY OF THE TROPICAL PACIFIC OCEAN

By Alexander Agassiz

Museum of Comparative Zoology,
Cambridge, Mass., *September*, 24, 1902.

My Dear Dr. Billings:

In accordance with my promise, I send you a short sketch of what might be accomplished by an expedition which would spend about three years in the tropical Pacific.

The points of interest are the hydrographic study of that area of the Pacific east of a line running from San Francisco to the Paumotus and from the Paumotus to the central part of Peru. With the exception of some work which I did in Central America off Panama and the Galapagos in 1891, from the Gulf of California to the northern part of Ecuador, nothing is known of the depth and character of the bottom of that great area. In connection with this work dredging should be carried on west of the area which I occupied into the deepest water of the eastern Pacific basin. The extension of the surface fauna in depth from the shore into deep water should also be investigated. The Central American coast north and south of the equator is admirably fitted for such study on account of its steep slope and the short distance to deep water from the coast.

Next, some time should be spent in dredging from the central part of each group of oceanic islands into deep water until one reached the abysmal fauna of the bottom of the Pacific to get thus some idea of the contrast there may be or the affinities, as may be the case, between these insular oceanic faunæ and the Pacific deep water fauna. The groups to be examined are Massason, the Society, the Paumotus, the Marquesas, the Cook, Samoa, Fiji, the Ellice group, Marshall and Carolines.

With this should be carried on deep sea tow net work and the study of the pelagic fauna associated with each group of islands. A study of the pelagic fauna should be made along the line of the equatorial and other great currents. The geology, botany, anthropology, and ethnology of these various archipelagoes should be studied. The time is coming, if it has not already come, when the natives of these various groups will have adopted the ways of modern civilization. It will then be impossible to learn anything of their former

language, habits, mode of life, customs, religion, and traditions unless they are studied within a reasonable time and before the flora has been modified by contact with civilization.

To carry out these investigations it will be necessary to have a twin screw steamer built for the purpose and properly equipped. The United States has no vessel adapted to that purpose. The *Albatross* is a single screw steamer and is altogether too small as a coal carrier to work with economy. Such a vessel should be 200 feet on the water line and have a beam of 32 feet; equipped it would probably cost $175,000. It would cost $75,000 a year to run the vessel. Three naturalists would be needed for the deep sea work. Two photographers and one artist would find ample occupation during the expedition. A couple of geologists, the same number of botanists and of ethnologists could during three years spent on these groups obtain information which probably could not be duplicated. If all this work could be under the supervision of one person it would be of great value. Previous expeditions have limited their work to a great extent to certain groups of islands, and in consequence what we know of the various groups is the result of the work of a number of individuals and not the résumé by a single person who has seen the whole. The scientific members of the expedition could be left at different groups, depending upon local transportation, and they could be picked up on completion of their work by the steamer and transferred to the next group. A period of three years' work would add immensely to our knowledge of the hydrography, the fauna of the Pacific, and of the great Oceanic Island groups.

The expenses of the scientific assistants would be from $20,000 to $25,000 a year, and the cost of publishing the results of this expedition, while depending upon the amount of material collected, would certainly, if done in a creditable way, involve an expenditure of $200,000 to $250,000 spread over a period of ten to fifteen years, or say an annual expenditure of $20,000 at the outside.

The above is a fair statement of work in an interesting field which I do not think any government is likely to take up. A German deep-sea expedition has lately returned from the Indian Ocean; the English after their expedition with the *Challenger* are not likely to enter the field again; the Prince of Monaco and the French have limited their work to the Mediterranean and the western coast of France, and no other nation is likely to enter into competition with us. The

United States will probably limit its marine work to what the Fish Commission can do with the *Albatross*, and, with the exception of some occasional outside work, the Fish Commission is hardly in condition to do more than attend to the problems that they have in hand.

On the termination of the expedition the steamer could do some physical and chemical work into deep water, either off the Atlantic or Pacific coast, in connection with a well equipped chemical laboratory on shore and finish accurately what can only be done roughly on board ship.

Yours truly, A. AGASSIZ.

BIOLOGICAL EXPERIMENT STATION FOR STUDYING EVOLUTION

BY ROSWELL H. JOHNSON

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NEED OF EXPERIMENTAL STUDY OF EVOLUTION.

Our knowledge of the processes of evolution has been greatly retarded by lack of experimental investigation. Nearly all of the post-Darwinian writing has been either largely deductive or else upon the variation of individuals at a particular time and place, *i. e.*, static. Evolution, above all other things, requires dynamic studies. Much dispute centers around such observation because of various causes which may have been operative. Experiment alone admits exclusion of possible interfering elements. The various points in dispute are nearly all capable of decisive experimental testing; for instance, selective life values of different variations, inheritance of acquired characteristics, environmental alterability of the germ-plasm; modification preventing death under adverse condi-

tions, etc. So clearly has this need been seen that hardly an evolutionary writer of prominence has not appealed for such work. I remember that Professor Davenport in his course on evolution at Harvard made a strong plea for experimental work.

It is gratifying to see some start now being made in this direction. Professor Ewart, of the University of Edinburgh, in Pennycuik farm is carrying on experiments in heredity with birds and mammals. The results on telegony are the most valuable ever produced, and have practically settled, for most biologists, that former moot question. De Vries, in Holland, is experimenting upon large series of plants in extensive gardens with reference to heredity and variation. His classic work, the *Mutations Theori* is founded upon the results of these experiments.

WHY SUCH STUDY HAS HARDLY BEGUN.

There is not lacking ability, interest, or desire to do work of this kind, but it is the lack of facilities which prevents effort. Such experiments are beyond the means of the unassisted worker for the following reasons:

(*a*) *Expense.* A barn, greenhouse, or a large and adequately protected garden is required. The collection of the material desired would sometimes require the expense of traveling.

(*b*) *Time.* Every day-year-around attention is impossible for most college teachers, who are generally absent for lectures, meetings and vacations, from time to time, and who can not afford to employ a reliable and skilled assistant to carry on the routine work in their absence.

(*c*) *Permanence.* Such experiments often need to be continued through many years. This is very difficult in a university where such work would probably be done by the energy of but one man who might at any time be called to another position.

(*d*) *The agricultural experiment stations are limited to economic problems.*

ADVANTAGES OF AN EXPERIMENT STATION.

It is, therefore, easy to see why such work has not been carried on in the past, except to a very small extent, and why we may expect to see valuable results from such an institution. A further consideration, however, is that effort in such vivarium must be vastly more productive than equal effort by individual investigators, because:

(*a*) Much of the labor involved is of a routine nature, which can be carried on by a moderately skilled gardener or attendant, and thus the results may frequently be of greater value, because experiments involving numbers may be prosecuted on a larger scale and more problems undertaken.

(*b*) *Superiority of Equipment.* The equipment would be of the best, thus insuring more and better results. Accidents resulting from improvised apparatus have spoiled many experiments, as witness the work upon breeding caterpillars.

The problems to be undertaken are numerous; the most important would seem to be those mentioned in the introductory paragraph.

A few specific cases may be briefly cited:

PROBLEMS TO BE SOLVED.

1. Environment alterability of the germ-plasms.

(*a*) Peas are said, by Bailey, to be particularly susceptible to the wetness of the soil in which they grow. Grow sweet peas which self fertilize in a heavy muck and keep well watered. Grow others in a loam never watered if outdoors. Grow the progeny in the respective environments for many generations; observe, first, what divergence takes place in one generation; and, second. to what degree, if any, the divergence is cumulative in successive years. *Each* year grow some plants from seeds of each set in an intermediate environment. Do they revert completely to a common type each year, or are they found to retain, each year, more of their divergence?

(*b*) Bring into cultivation some wild plants of a species which has given rise to a cultivated variety. Carefully avoid selection. Observe whether successive generations, nevertheless, diverge from the wild species.

2. Inheritance of somatogenetic characteristics.

(*a*) Some species, as the painted turtle, which both swims and walks and has short generations, may be experimented upon in two series. In one case they are confined to water; in the other, kept from it. Observe as above.

(*b*) Grow Chologaster, from which the blind fishes are descended, in the dark. Observe as above.

3. Isolation.

(*a*) Isolate two groups, of ten each, of a species which differ abnormally in opposite directions from the normal conditions, *e. g.*, ten turtles lacking one pair of scutes and ten turtles with a supernu-

merary pair. In successive generations would they regress to the mean, according to Galton, or diverge, according to Gulick?

(*b*) Experiment similarly with two groups, not far from the mean, of some very variable species, as with fish with anal fin rays having a normal distribution from 8-14 with mode at 11, using the one group with 10, the other with 12. Observe as above.

4. Natural selection.

The life-death value of variations. Subject large numbers of *Palaemonetes vulgaris* (a marine species) to fresh water. Compare the survivors with the dead, in respect to number of rostral spines, because a fresh water species of the genus differs in having fewer spines, as do the races of this species from brackish water of very low salinity.

5. Fecundal selection.

(*a*) Grow a large series of several varieties of wax podded beans, which self fertilize. Record fecundity. Is it correlated with any other characteristics? In the first series grow seed from the five most fecund plants each year; in the second, grow seeds from five plants of mean fecundity. Do the series diverge in other respects than fecundity?

(*b*) Do the number of visits of insects vary throughout the flowering period of a species? If so, does the fecundity of the successive flowers correspondingly vary? Sow from the most fecund flowers and from those of average fecundity. Does the time of flowering diverge in the two series? Is any correlated character affected?

(*c*) In a racemose species, do the lowest flowers differ from the highest in fecundity? If so, grow seed from each. After several generations of such selection, do the plants differ?

(*d*) Same with the inside and outside florets of a sun flower.

6. Organic evolution.

A tree frog which has the power of very slowly acquiring the color of its environment may be grown in a green environment.

Those which become the greenest would be artificially selected, in the successive generations.

In another lot, grown under ordinary conditions, select the greenest in the successive generations.

Is the green color acquired more quickly in the first case, when the selection is along the line of individual accommodation, than in the second case?

7. Sport prepotency.

(*a*) Cross an albino of a species of a genus which has white species with the normal form; observe potency of the sport.

(*b*) Isolate an albino with nine normal individuals, four of same sex as albino and five of the other.

After many generations is the white swamped or does it prevail?

8. Do likes tend to breed together, thus establishing segregation and divergence?

This could be observed in Leghorn fowls with rose comb and with single comb; also with Buff fowls which have many white tail and wing feathers and those which are almost free from them.

9. Correlation.

Select solely for one character. Are many correlated characters also intensified?

10 Determinate variation.

Grow one species, like *Oenothera lamarckiana*, known to have determined variation, permitting all seeds to grow or selecting at random for successive generations. Do the "*petites especes*" become more prominent?

Caution in all experiments. Record all deaths, in order that any influence of natural selection in the results may be recognized.

Equipment and Cost.

The Vivarium would, in general aspects, resemble an agricultural experiment station, and could be creditably and efficiently run within the limits of the size and cost of the several agricultural stations of the country. I shall outline what I have in mind, although either retrenchment or enlargement would be possible within certain limits, affecting only the amount and variety of results.

(*a*) *Greenhouse.* This should be divided to give three temperatures, a large middle house with two smaller portions partitioned off for cooler and hotter temperatures. All three portions should be large enough to give space for aquaria, insect cages, and the like, as well as for plants.

(*b*) *Barn.* This should be long, narrow, and one story high. On either side of the aisle would be wired cages, each communicating with wired runways outdoors, designed for cats, rabbits, guinea-pigs, or fowls.

(*c*) *Laboratory building*. This should have two stories and a basement, and should adjoin the barn and greenhouse. This would contain a general work and store room and several laboratory rooms.

The construction of all buildings should have permanence its chief end; hence they should be fireproof throughout. The initial equipment would consist of gardening and greenhouse tools, microscope, microtome, reagents, photographic apparatus, measuring apparatus, computing tables, instruments, glassware, trays, aquaria, wire cages, etc. Other equipment that would be needed as the experiments demanded would be seeds, plants, animals, feed, chemicals, etc.

LOCATION AND GROUNDS.

The laboratory should be situated on a protected marine shore, near a fresh water pond. The grounds should contain a garden of two acres and cement walled ponds. The location should be far enough south to have a long growing season and to economize fuel. Cheap lands and good collecting grounds necessitate a country location, but it should be situated within easy range of a large city, preferably Baltimore, where purchases can be quickly made and the necessary books loaned or consulted. I consider that $10,000 would cover the erection and original equipment.

MAINTENANCE.

The regular staff would consist of a director and an assistant director, who would be men of scientific attainments in this line, and also of two attendants, one who has skill as a gardener and the other as a care taker of animals.

The plans provide two extra laboratory rooms on the second floor for additional workers $1,800, $1,500, $800, and $700 might, I think, suffice as salaries. Another $1,500 a year would be necessary for materials needed and for minor expenses, bringing the annual maintenance expense to $6,300. Doubtless the results of the work could be published in existing journals without expense to the Vivarium.

TIME.

One year should suffice to complete the building and begin operation.

STANFORD UNIVERSITY, CAL., *August* 28, 1902.

To the Board of Trustees of the Carnegie Institution.

GENTLEMEN: I have received a copy of an outline of a plan for a proposed Vivarium for Experimental Evolution, suggested by Mr. Roswell H. Johnson, of the University of Wisconsin.

Permit me to say that, in my judgment, the establishment of such an institution, if placed in charge of some man of high skill in handling this class of experiments, would be an extremely desirable piece of work in the line of advanced research, and I trust that the Committee of the Carnegie Institution in charge of this matter will give it careful consideration.

Very truly yours,

DAVID S. JORDAN.

BIOLOGICAL EXPERIMENT STATION FOR STUDYING EVOLUTION

BY CHARLES B. DAVENPORT

ZOOLOGICAL LABORATORY.
UNIVERSITY OF CHICAGO, *May* 5, 1902.

To the Trustees of the Carnegie Institution.

GENTLEMEN: I beg leave to present for your consideration the following proposal for the establishment and maintenance by the Institution of a Biological Experiment Station for the study of evolution.

1. *Aims.* The aims of this establishment would be the analytic and experimental study of the causes of specific differentiation—of race change.

2. *Methods.* The methods of attacking the problem must be developed as a result of experience. At present the following seem most important:

(*a*) Cross breeding of animals and plants to find the laws of commingling of qualities. The study of the laws and limits of inheritance.

(*b*) The experimental production of variation, both by internal operations, such as hybridization, or by change of external conditions.

(*c*) Study of normal variation, especially as associated with changes in habitat and with geographical distribution—the effects of isolation.

(*d*) Experimental study of the effects of selection and of the origin of adaptation in organisms. Does morphological adaptation precede or follow change of habitat?

3. *Importance.* The scientific need of such a station has long been recognized. Bacon recognized its importance and dreamed of it in the new world. In later times Whitman and Osborn in this country, Romanes in England, and De Varigny in France have pointed out the scientific necessity for such an institution. Our agricultural experiment stations fall far short of meeting the scientific requirements, because their work has to be of an immediate practical turn. The Carnegie fund offers the opportunity for which the world has so long been waiting.

4. *Requirements.* To carry out the program proposed the following will be required:

(*a*) Plot of ground in the country, near the sea, presenting a great variety of conditions, not too distant from a scientific center with its libraries.

(*b*) Building provided with a greenhouse, constant temperature, and dark underground rooms, aquaria, insectaries, mammal rooms, administrative offices, and laboratories.

(*c*) Scientific staff, consisting of a director and two assistants; also two or more laborers.

5. *Estimate of Expenses:*

Initial.

Cost of ground, say 20 acres, about	$10,000
Cost of laboratory buildings and furnishings	25,000
Cost of putting grounds in order, seeding, fencing etc	500
First cost of stock	500
Total initial expense	$36,000

Annual.

Salary of Director	$4,000
Salary of two assistants	2,000
Wages of laborers	1,200
Feed	300
Fuel	250
Books, stationery, etc.	750
Repairs, additions, and laboratory supplies	500
Total annual expense	$9,000

6. *Specific Proposals.* That this station be established at Cold Spring Harbor, which has the advantages of abundant fresh water, proximity to the sea, good sanitary conditions, great variety of environments, proximity to New York, to the Biological Laboratory, and to the New York Fish Hatchery.

Respectfully submitted,

CHARLES B. DAVENPORT.

ESTABLISHMENT OF A BIOLOGICAL FARM

BY DR. C. O. WHITMAN

The following is quoted from the report of Dr. C. O. Whitman, on a biological farm in connection with the Marine Biological Laboratory at Woods Hole, Massachusetts.

The fundamental problems of heredity, variation, and evolution can not be wholly considered in the laboratory. They concern vital processes known only in living organisms—processes which are slow and cumulative in effects, expressing themselves in development, growth, life histories, species, habits, instincts, intelligence. These problems require, therefore, to be taken to the field, where the forms selected for study can be kept under natural conditions, and where the work can be continued from year to year without interruption. Such a field, combining land and water, and stocked with animals and plants, and provided with a staff of naturalists, would have the essentials of a biological farm, now justly considered to be one of the great desiderata of biology.

This great need has been felt ever since Darwin's time, and has been strongly urged by such evolutionists as Romanes, Varigny, Galpon, Weismann, and Mendola. Thus far the project has not been realized, except on a small scale, through individual effort.

The functions to be fulfilled by a biological farm are the deep and broad needs of pure research on living organisms. The problems of heredity and variability are fundamental and naturally form the center of interest. Variability is the source of new species and the fountain of all progressive development in the organic world. In heredity lies the power of propagation and continuity of species. These are inexhaustible subjects, from the investigation of which must flow rich accessions to knowledge, which will redound to the

advancement of human welfare. The functions of a biological farm are not all summed up in experimentation. That old and true method of natural history observation must ever have a large share in the study of living things.

The biology of today is not too much laboratory, but too little of living things.

Outlay and maintenance.—The original outlay for land, stock, buildings equipment, inclosures of land and water for isolation purposes, would vary according to the forms selected for study. From $50,000 to $100,000 would suffice for this. The maintenance of the first section, including salaries, accessions to stock, library, etc., may be estimated at $10,000 a year. The cost of additional sections would be about $5,000 each.

PROPOSED ANTARCTIC EXPEDITION

CARNEGIE MUSEUM, PITTSBURG, PA.,
April 21, 1902.

To the Executive Committee of the Carnegie Institution.

GENTLEMEN: Inasmuch as the same generous donor who has called into being the institution which you represent is the founder for like ends of the institution which we have the honor of representing, we are gratified by the receipt of your letter asking for suggestions as to any special line of scientific research to which in our judgment a portion of the funds of the Carnegie Institution might be advantageously devoted.

In reply to your communication we beg to submit the following as worthy of your earnest consideration:

The Carnegie Museum of Pittsburg, Pa., is desirous of despatching an expedition, under the leadership of Mr. J. B. Hatcher, to the Antarctic regions, and more particularly to Graham Land and the adjoining seas and islands.

The chief purposes of this expedition will be, *first*, to study the geology, paleontology, and biology of the lands and waters of this region, with special reference to their bearing upon the supposed former land connection between South America and Antarctica, and to discover, if possible, the nature, duration, and time of such connection; *second*, to make as complete collections as possible of the marine and terrestrial vertebrate and invertebrate faunas

and of the floras of this region; *third.* to conduct two lines of soundings, one from Cape Horn to Graham Land by way of the Dirk Garritz archipelago, and a second from South Georgia by way of the South Orkneys. It is proposed also to devote considerable attention to the geology and biology of the South Orkneys, South Georgia, and the Fuegian archipelago and their waters.

For the proper accomplishment of the work of this expedition it is proposed to place the geological and paleontological work under the direct charge of the leader of the expedition, and to engage the services of competent invertebrate and vertebrate zoologists to take direct charge of the work in those departments, supplying each head of these three departments with experienced assistants. The salaries of the scientific staff of the expedition will be met by the funds of this museum.

For transporting the expedition to and from the Antarctic region and for facilitating its labors while in that region, it is proposed to purchase, or charter for a period of two years, commencing August 1, 1903, an efficient and serviceable steam whaler or other vessel, and to fit her for deep sea soundings and for dredging to moderate depths, say from 500 to 800 fathoms. She should also be provided with suitable laboratory facilities for the zoologists of the expedition.

In order that this work may be carried out successfully and with efficient funds at its disposal, we, the undersigned officials of this museum, respectfully request that on August 1, 1903, a fund of $10,000 be set aside for securing by charter or purchase a suitable vessel and providing the necessary equipment of the same.

And, further, that on the first day of August, 1904, and on the same day of the same month, 1905, two further sums of $25,000 each be set aside for the expenses of the expedition so far as they may be included in chartering, purchasing, equipping, and maintaining the vessel, paying the crew, and in general conducting the expedition; all amounts to be accounted for and any balance not expended to be returned to the treasury of the Carnegie Institution.

C. C. MELLOR,
Chairman of Committee on Museum of the Board of Trustees of Carnegie Institute.

W. J. HOLLAND,
Director of the Carnegie Museum.

J. B. HATCHER,
Curator of Vertebrate Paleontology.

PROPOSED INVESTIGATION OF SUBTERRANEAN TEMPERATURES AND GRADIENTS

By G. K. Gilbert

Washington, D. C., *November 19, 1902.*

To the Trustees of the Carnegie Institution.

Gentlemen: I beg to submit for the consideration of the Institution a proposed investigation of subterranean temperatures and gradients by means of a *deep boring* in plutonic rock.

Information as to temperature gradients in the earth's crust depends, up to the present time, on observations made in wells bored for economic purposes and in mines. Wells bored for economic purposes penetrate the sedimentary rocks, because such rocks contain the potable waters, brines, petroleums, gases, and salt deposits which are sought. For various reasons these rocks should not be expected to give the normal temperature gradient of the earth's crust. First, they are heterogeneous, and as different layers differ in heat diffusivity, the gradient is modified thereby; second, the temperatures of the sedimentary rocks are, as a rule, modified by the circulation of water; since heat is thus distributed by convection, the conditions are not favorable for the determination of the facts of conduction; third. the sedimentary rocks are not representative of the crust as a whole; therefore inferences from their temperature gradients and other heat phenomena can not be applied to the igneous or plutonic rocks which make up the body of the crust.

Temperature observations in mines are also subject to exceptional conditions, since the ore deposits followed by deep mines are often associated with comparatively recent diastrophic or volcanic disturbances, and are usually routes of water circulation.

The student of the problems of the inner earth needs information as to the thermal conditions of a homogeneous rock mass representative of the crust as a whole. To this end it is desirable that a boring be made, as deep as possible, in a plutonic rock. A mass should be selected which is of great age and which has not for many geologic periods been subjected to diastrophic changes. A mass having large superficial extent and presenting uniform characters throughout its area could presumably be penetrated to a great depth without encountering important changes in composition.

The method of boring adopted should be one which yields a core, so that physical investigations can be made of the rock penetrated

at various depths. Rock specimens from the core should be examined in the laboratory to determine their heat diffusivity, and this should be investigated (1) in relation to varying pressure, and (2) in relation to varying temperature.

The results to be hoped from such an investigation are:

1. A determination of the general temperature gradient of the upper part of the crust—a determination having higher authority than any previously made.

2. A determination of the downward variation of temperature gradient.

3. A redetermination of the rate at which heat reaches the surface through conduction in the crust.

4. A determination of any variation in the rate of heat movement in relation to depth. The character of such determined variation, or the determination of the absence of variation, bears on theories as to the source of the heat conducted toward the surface.

5. The detection of variations of gradient occasioned by Pleistocene oscillations of climate, and their possible interpretation in terms of time.

The value of such a research will depend largely upon the depth penetrated by the boring. I am informed by Mr. F. H. Newell, of the United States Geological Survey, that core drilling has heretofore been carried only to moderate depth, so that for the purposes of such an investigation a special plant, of an elaborate character, will be required. This plant should provide for the commencement of the boring with a large diameter and the progressive reduction of diameter as greater depths are reached. The plant should be carefully planned in advance, and such planning requires expert knowledge and skill. Mr. Newell informs me that the necessary knowledge and skill can be commanded, and mentions in that connection Professor Slichter, of Madison, Wisconsin.

I am not prepared to make recommendation as to persons by whom the proposed investigation should be conducted; but if the Institution shall establish a laboratory of geophysics the general direction of the investigation might advantageously be entrusted to the officers of that laboratory.

I estimate that the amount of money needed for the work will be $50,000, that the time probably covered would be three years, and that the need for the first year would be $10,000.

Very respectfully,

G. K. GILBERT.

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